SCIENCE

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RECENT EVIDENCE FOR THE EXISTENCE OF THE NUCLEUS ATOM[1]

THE great French scientist Poincaré, just before his death two years ago, described an atom before the French Physical Society in these words:

> Each atom is like a kind of solar system where the small negative electrons play the rôle of planets revolving around the great positive central electron which takes the place of our sun. . . . Besides these captive electrons there are others which are free and subject to the ordinary kinetic laws of gases. The second class are like the comets which circulate from one stellar system to another, establishing thus an exchange of energy between distant systems.

Such an atom is a world in itself and strangely different from the kind we learned about in our text-books twenty years ago. One of the much used chemistries of that day put it in this way:

> An atom is the smallest portion of matter that can exist; it is incompressible, indivisible and in itself unchangeable.

How has this great change of view come about? How has the indivisible unit evolved into the complex microcosm we now imagine? Time would fail us to trace all the steps of the way; we will attempt only to bring out some of the considerations which have in the past three years led many of our foremost thinkers to believe in that particular type of atom which we may call the nucleus atom. This type is similar to that which Poincaré pictured except that the central body is much smaller—very

[1] Address of the vice-president and chairman of Section B, Physics, of the American Association for the Advancement of Science, at Philadelphia, December 29, 1914.

small indeed as compared with even the minute electrons which circulate about it.

We will recall first several of the discoveries which have forced us to abandon the idea of an indivisible atom. The fundamental one was Sir Joseph Thomson's discovery of the electron. In studying the nature of the cathode rays he found that they consisted of extraordinarily minute particles all exactly alike, whatever the nature of the gas within the tube might be. In a series of brilliant experimental studies he was able to show that the mass of one of these electrons was only one eighteen hundredth that of the lightest known atom. Then came Zeeman's discovery[2] that the separate lines of many spectra are broken up into two or more lines by the action of strong magnetic fields. The study of this effect made it quite certain that light radiation is caused by the rapid vibration of electrons in the luminous body. Therefore electrons must be present in very many kinds of matter—probably in all. The electrons were early proved to carry a negative charge of electricity. Soon they revealed their presence in a great variety of ways and assisted in the explanation of widely different phenomena. But the corresponding positive constituent of matter proved singularly elusive although most diligently sought for, and it is only very recently that we seem to have traced it to its hiding-place.

Different views regarding the nature of this positive constituent have led to much diversity of opinion regarding the structure of atoms. One of the most successful of these theories is that proposed by Sir Joseph Thomson in 1904.[3] He supposed a relatively large positive mass to exist—nearly as large as the atom—with the minute negative electrons distributed through it in such a way as to make the system a stable one. For easy mathematical treatment he assumed the electrons at equal distances apart in a series of concentric circular rings. To secure stability and illustrate certain atomic properties he supposed these rings to be in rotation. Thomson discussed many such configurations and satisfactorily explained many facts regarding the valency, the position in the periodic system, the electropositive or electronegative character and other chemical properties of different substances.

A modification of Thomson's atom was proposed by H. A. Wilson in 1911.[4] He supposes each negative electron to be situated at the center of a positive sphere of sufficient size to neutralize it electrically, and the atom to be made up of a group of such units, the total number being proportional to the atomic weight. In other words, Thomson's one relatively large positive mass is divided up into equal parts, each one containing a single negative electron. The mathematical development of this idea led to the result that the hydrogen atom contains eight such units. The gold atom would therefore contain about sixteen hundred of them.

In the Thomson and the Wilson atoms, the positive portion is diffused throughout nearly the whole volume of the atom, a region about one hundred millionth of a centimeter in diameter. This type of structure has accounted for many atomic properties but has not been very successful in explaining the position of the lines in light spectra caused by vibrations in the atom.

I wish to direct your attention to-day more particularly to a type of atom in which the positive charge—equal as before to the sum of the charges of the negative electrons—is highly concentrated at the center of volume of the atom, occupying only an exceedingly small part of the volume. Nagaoka[5] had discussed the stability of such an atom in 1904. Sir Ernest

Rutherford revived it in 1911 to explain phenomena observed by Geiger and Marsden,[6] and achieved a striking success. The facts observed were these; when α-rays were allowed to pass through thin sheets of metal, a small proportion of them were observed to be deflected through very large angles. Rutherford[7] made a theoretical examination of the results of a single encounter between an α-particle and an atom of the concentrated-nucleus type, and calculated the proportion of the α-particles which would be deflected through various angles by such encounters. Geiger[8] then made a new experimental study of the scattering produced by gold foil and found a very satisfactory verification of Rutherford's formula. From the amount of scattering at various angles, the value of the nucleus charge was also calculated. For gold it came out about 100 e. The general conclusion was reached that the nucleus charge is about one half the atomic weight times the charge of an electron. But Barkla[9] had earlier reached the same value for the sum of the electron charges—which in a neutral atom should equal the nucleus charge—by observations on X-rays and the use of a theory developed by J. J. Thomson. According to these views atoms contain only about one sixteenth as many electrons as they do on the theory of H. A. Wilson.

On the assumption that large angles of deflection are sometimes due to single encounters with an atom, large forces must be postulated to swing the α-particles so considerably from their paths, forces so large as to require an approach to within an exceedingly small distance from the nucleus center. This distance was calculated to be about 1/3,000 of the atom diameter. If this is true, the nucleus can hardly have a diameter exceeding 1/5,000 that of the atom.

The view that an α-particle may turn through a large angle as the result of a single encounter was strikingly confirmed in 1912 by some remarkable photographs of the paths of α-particles through a gas, taken by C. T. R. Wilson.[10] I have here a reproduction of one of these photographs which shows two abrupt bends in the trail of a particle, one of 10.5° and the other of 43°. This second bend would certainly seem to be a case of "single scattering." The astonishing conclusion regarding the small size of the nucleus has been confirmed by some recent experiments of Marsden[11] in passing α-radiation through a gas.

A theory had been worked out by Darwin that when α-radiation entered hydrogen, a few H atoms would acquire from close encounters with the α-particles a velocity 1.6 times that of the striking α-particle, corresponding to a range four times that of the radiation. Marsden's experiments were undertaken to test this theory. He passed α-rays into hydrogen and observed the scintillations on a zinc sulphide screen placed at various distances. The range of the α-particles was found to be 20 cm., but a few scintillations were found when the screen was as much as 90 cm. distant, due seemingly to the rapidly moving H atoms in their recoil from collision with the heavier α-particles. This was a striking confirmation of Darwin's theoretical calculations. Calculation by his method showed that the centers of the nuclei during collision were not over 1.7×10^{-13} cm. apart. This then would be the maximum value of the sum of their radii. This is smaller even than the former result and also smaller than the accepted value of the diameter of an electron.

Thus the nucleus of the atom appears to be extraordinarily minute, and this suggests an explanation of the somewhat paradoxical result, that practically all of the mass of

the atom seems to reside in the nucleus. For if the size is extremely small its electromagnetic mass would—from the formula $2/3\frac{e^2}{a}$—be relatively large. So its mass might be 1,800 times that of the electron (and J. J. Thomson's experiments suggest that no positive carrier has a mass smaller than that amount) provided its diameter were only 1/1,800 that of the electron. From such consideration Rutherford[12] thinks it probable that the *nucleus of the H atom* is, in fact, the long-sought positive electron.

Attention has been forcibly drawn to the nucleus type of atom within the past year and a half by the extraordinary success it has had as interpreted by Bohr, Darwin and Moseley, in accounting for the exact position of lines in the spectra of gases. Their work has also served to bring into the limelight the earlier and perhaps equally striking work of J. W. Nicholson. In November, 1911, he published a paper[13] in which he assumed the existence of several elements with atoms of very simple and definite structure. One of these he called nebulium. In the neutral condition it was supposed to have a positive nucleus with charge 4 *e*, and around it at equal distances apart in a circular path, rotated four electrons each with unit charge *e*. It might, however, lose one electron, when it would become positively charged, its three electrons now taking up new positions a third of a circumference apart. Similarly he supposed that the atom might take up more electrons, and have a negative charge.

He discussed mathematically the vibratory motions of such an atom and showed what kind of a spectrum the radiation would furnish. The theoretical analysis of the spectrum of his imaginary element nebulium showed that all the characteristic nebula lines of the Great Nebula in Orion, leaving out those due to hydrogen and helium, could be attributed to the vibrations of the nebulium atom, except two lines. On the very day he read this paper in England a German astronomer, M. Wolf,[14] presented a paper in Heidelberg which described the discovery that different lines of this nebula were due to radiation from different parts of the nebula, and that these two lines which Nicholson had found exceptional were due to a radiating source different from that of the other lines. Whereas almost all the lines were due to radiation from the bright ring of the nebula, these two lines were caused by radiation from different parts of the nebula, that for one of them coming from the dark central space and for the other chiefly from the outer edge of the ring. All other lines had their maximum brightness in the bright ring itself.

Another imaginary substance which Nicholson named protofluorine, he succeeded in connecting in a similar way with the spectrum of the sun's corona.[15] This atom he supposes to have—when neutral—a nucleus 5 *e* with 5 electrons in a circular orbit about it. He analyzes its radiation on the assumption that it gives forth radiation energy in quanta, as Planck has supposed. He anticipates Bohr in the emphasis he gives to the idea of constancy of angular momentum in the rotating electrons. His calculations on this protofluorine atom account satisfactorily for the existence of fourteen out of the twenty-two lines of the corona spectrum, with an average difference of less than one part in a thousand between observed and calculated values. His calculations also show the magnitude of the positive or negative charge of the atoms originating the various lines. He concludes that in these primitive forms of matter—nebulæ and

solar corona—very simple types of atom exist, much more simple doubtless and more amenable to calculation than are the atoms of most terrestrial substances. While the correspondence between his calculated spectra and those observed at Lick Observatory is not so close as is that between theory and observed spectra in the recent work of Bohr, it is important to observe that most of these results are obtained by means of established mechanical principles and without the use of such questionable assumptions as the brilliant young Dane cheerfully and confidently makes.

And now let us consider briefly the work of Bohr. This is set forth in four papers[16] published in the *Philosophical Magazine* between July of last year and March of the present year. He starts with the Rutherford atom, *i. e.*, a minute positive nucleus with its system of electrons revolving about it, the mass of the atom resident chiefly in the nucleus and the number of electrons approximately equal to half the atomic weight. He admits the difficulty of securing stability in such an atom (as compared, for instance, with Thomson's 1904 atom), but thinks that this difficulty can be removed if we admit the insufficiency of the classical dynamics to explain phenomena involving atomic distances, and introduce Planck's quantum into the equations. He claims that this furnishes a basis not only for a theory of atomic constitution but for that of molecules as well. He differs from Nicholson radically in assuming that when in a state of uniform rotation, the electrons do not radiate. This is not in accordance with our ordinary electrodynamics. Each atom, according to Bohr, has a number of "steady states" during which the electrons revolve uniformly and there is no radiation. But in passing from one steady state to another an electron winds inward towa the nucleus with its frequency increasing. Its acceleration meanwhile causes radiation, until the electrons settle into another steady state and ceases for the time to radiate. In its stable state the angular momentum of every electron is the same. This agrees with Planck's idea of discontinuous radiation and the amount radiated in one emission for a vibrator of frequency ν is $\tau h\nu$ where τ is some integer and h is Planck's "universal constant." Bohr finds the equation for the relation between the frequency, mass of an electron, charge of electron, τ and h. When τ is made 2 in the equation, Balmer's series for hydrogen is obtained, and for $\tau=3$ the infra-red series which Ritz anticipated and Paschen found. $\tau=1$ gives a series of lines in the ultra-violet and $\tau=4$ and 5 in the infra-red, neither of which has yet been observed. The lines observed by Fowler and by Pickering he connects with helium instead of with hydrogen.

From this equation he also calculates Rydberg's number $N°$ and obtains 3.26×10^{15}. Its observed value is 3.29×10^{15}, so that the agreement of theory with observation is satisfactory. The theory further requires that very low gas density be required for numerous spectrum lines and very great gas volume for sufficient intensity. This probably accounts for the fact that 33 lines of the Balmer series for hydrogen can be seen in celestial spectra while only 12 appear in terrestrial (vacuum-tube) spectra.

From the work of Barkla, and of Geiger and Marsden on the scattering of radiation Bohr accepts the view of van der Broek that the number of electrons in an atom in the neutral state indicates the position of the element in the periodic table. Thus he gives hydrogen one electron, helium two, lithium three, beryllium four, etc. The same number expresses the magnitude of the positive charge on the nucleus.

It is difficult to pass upon the validity of

some of Bohr's assumptions. So high an authority as Jeans[17] calls it "a most ingenious and suggestive, and I think we must add convincing explanation of the laws of series spectra," and yet he adds a little later that the only justification for the assumptions Bohr makes is "the very weighty one of success." Rutherford cautiously observes:

> The theories of Bohr are of great interest and importance as a first attempt to construct atoms and molecules and explain their spectra.

The views of Rutherford and Bohr regarding the structure of atoms are strongly supported by some striking experiments of Moseley published during the past year.[18] His work utilizes the methods worked out by W. H. and W. L. Bragg[19] for measuring the spectra obtained by reflecting X-rays from the faces of crystals. Barkla and Sadler[20] showed in 1908 that if X-rays from an ordinary tube fall on different metals, "characteristic X-rays" are given off—these being different for each metal. Many metals can give out at least two different types of radiation. Barkla called these the "K series" and the "L series" radiations. For each metal the "K" radiation is about 300 times as penetrating as the "L" radiation. Kaye[21] has shown that an element excited under suitable conditions by rapid cathode rays gives out a considerable portion of the X-rays produced in the form of characteristic rays.

Moseley photographed the spectra obtained by using a great variety of different metals as targets for cathode-ray bombardment. The X-rays so produced were reflected from a crystal face and then fell upon the photographic plate. Spectra of the third order showing fine sharp lines were obtained. Similar results were secured for over forty metals. For the elements of lower atomic weights, each spectrum showed two prominent lines, and the spectrum of any element was almost exactly like that of the element next below it in the periodic table except that it was shifted in the direction of shorter wave length by about the distance between its two lines. The radiation was of the "K" type. Thus a close relation was established between the X-ray wave-length and chemical properties. Further, the frequency of the principal line was found to be proportional to $(N\text{-}a)^2$, where N is an integer and a is a constant equal to about unity. N is called the atomic number of the element. Thus is it 20 for Ca, 22 for Ti, 23 for Va, 24 for Cr, 25 for Mn, 26 for Fe, 27 for Co, 28 for Ni, 29 for Cu, 30 for Zn, etc. These numbers are very nearly in the orders of the increasing atomic weights, but more exactly in the order of Mendeleeff's periodic table. The numbers then correspond with the changes in chemical properties more nearly than do the atomic weights. For instance, we have Fe, Co, Ni representing both the chemical order and order of the atomic numbers (26, 27, 28), while Fe, Ni, Co is the order of increasing atomic weights. It thus appears that this atomic number is a more fundamental quantity than is the atomic weight, or as Soddy[22] has put it,

> It is the nuclear charge rather than the atomic mass, which fixes the position of the element in the Periodic Table.

A. van der Broek[23] had before this suggested that the total number of unit charges on the electrons of an atom is the number representing the position of the element arranged according to increasing atomic weight. But in a neutral atom the sum of the (negative) charges on the electrons should equal the positive charge on the nucleus, so that the two statements amount to the same thing.

When the experimental values found for the frequency were compared with those

indicated by Bohr's theory, the agreement was found to be a remarkably close one.

With elements of higher atomic weight Moseley obtained spectra whose lines indicated the Barkla "L type" of radiation. The atomic numbers calculated from the positions of the strongest lines of these "L" spectra ranged from 40 for zirconium to 79 for gold. These experiments then give strong support to the hypothesis of van der Broek that the total charge of the electrons of an atom indicates its position in the periodic system. Known elements were found to correspond with all the numbers from 13 to 79 except three, indicating that three elements probably remain to be discovered. The wave-lengths of the characteristic X-rays from the metal is of the order of 1/1000 that of visible light (*i. e.*, about 40 waves in .000001 inch).

During the past few months Rutherford and Andrade[24] have extended these methods of crystal reflection to the study of radiation from Ra-B Ra-C. The γ-ray spectrum of Ra-B was found to be of the same general type as that of the X-ray spectrum from various heavy metals when bombarded by cathode rays. The result for soft γ-rays from Ra-B shows that its radiation belongs to the "L series" for heavy metals. Moseley's formula applied to the measurement of the lines of the γ-ray spectrum gave $N = 82$, which is the atomic number of lead. The atomic weight of Ra-B is, however, 214, while that of lead is 207. This difference is nevertheless fully explained by a new generalization of Soddy and Fajans which we will presently notice. The experiments described in the second paper were made with much more penetrating γ-radiation from both Ra-B and Ra-C. This penetrating γ-radiation from Ra-B was found to correspond to the K series for the same metal, lead. The still more penetrating radiation from Ra-C has a line spectrum of still higher frequency than the K type, for which the name "H" series is suggested. These rays are especially interesting because they have by far the shortest wave-lengths yet known, only about 1/8 of the wave-length of the shortest X-ray waves measured by Moseley or about 1/80,000 of the wave-length of sodium light. Rutherford in his comments on these waves very justly remarks, "It is surprising that the architecture of the crystals is sufficiently definite to resolve such short waves."

During 1913 some remarkable work on the relations of radioactive substances to each other has given support to the nucleus atom from an unexpected quarter. Fleck,[25] Russell,[26] Von Hevesey,[27] Fajans[28] and Soddy[29] have all had a share in this work. They have found that when a radioactive substance ejects an α-particle a substance of different chemical properties and different valency results. The new substance lies two columns to the left in the periodic table, has an atomic number two less and an atomic weight about four less than the parent substance. If however the radioactive substance ejects a β-particle or electron, the new substance is one column to the *right* in the periodic table, increases one in atomic number, and does not change in atomic weight. Plainly then two or more elements may occupy the same position in the periodic table, for if an element loses in succession—in any order—two β-particles and one α-particle, its atomic number will be again the same as it was at first. Thus Ra-D has the atomic number 82; it loses a β-particle and becomes Ra-E with atomic number 83; this loses another β-particle and becomes Ra-F with atomic number 84; this finally loses an α-particle and becomes lead, with the original atomic number 82. The series Ur1, Ur X 1, Ur X 2 and Ur2 is of the same kind, except that the particles are ejected in the reverse

order, α, β, β. So the old difficulty of finding places in the periodic table for the 34 radioactive substances now known has disappeared, since they have but ten different atomic numbers and require therefore but ten places in the periodic table. Soddy has introduced the term isotopes to designate two elements occupying the same place in the table. Isotopes are chemically inseparable and probably have identical spectra, but they have different atomic weights.

It is evident that much remains to be done before we have very definite ideas of the structure of the nucleus atom. Many questions are entirely unanswered. For example, in how many rings do the electrons lie? For hydrogen and helium as for nebulium and protofluorine (if they exist) the electrons are so few that they doubtless all lie in one ring, but there are reasons for believing that in atoms of higher atomic weight there are two or more rings. With a large number of electrons present—with the 100 electrons of the gold atom for instance—there may indeed be several configurations which will satisfy the conditions of stability. Even for comparatively light atoms Bohr[30] supposes that as many as five rings exist. Again from what part of the atom of a radioactive substance do these ejected α- and β-particles come? Soddy[31] believes that both originate in the nucleus, but that the chemical and the electro-chemical properties are controlled by the outer ring of the electrons. Moseley regards the similarity of the X-ray spectra of different metals as satisfactory evidence that such radiation originates inside the atom, while light radiation is determined by the "structure of the surface." Rutherford[32] and Bohr both raise the important question whether atomic nuclei contain electrons, and both conclude that they do. These and many other questions have already been asked but only tentative and provisional answers have thus far been given. Doubtless there is a field here for much important experimental and theoretical work in the immediate future—a field which American physicists will seek to cultivate with their European brethren, who have done about all of the work thus far.

These hasty considerations perhaps suffice to show the varied character of the lines of evidence that have been developed during the past three years to give support to some form of nucleus atom. Radioactive phenomena, X-ray radiation and chemical properties seem to give united testimony for it. Doubtless the final type of atom has not yet been described, for it is easy to criticize the views of Nicholson, of Bohr or any other who has proposed a model, but it is probable that some form of nucleus atom will soon receive general recognition.

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11. E. Marsden, *Phil. Mag.*, 27, 824–830 (May, 1914).

12. E. Rutherford, *Phil. Mag.*, 27, 488–499 (Mar., 1914).

13. J. W. Nicholson, *Roy. Astron. Soc. M. N.*, 72, 49–64 (1911).

14. M. Wolf, *Nature*, 89, 70 (Mar. 21, 1912).

15. J. W. Nicholson, *Roy. Astron. Soc. M. N.*, 72, 139–150 (1911).

16. N. Bohr, *Phil. Mag.*, 26, 1–25 (July,

1913); 476–502 (Sept., 1913); 857–875 (Nov., 1913); 27, 506–524 (Mar., 1914).

17. J. H. Jeans, *Report B. A. A. S.*, Birmingham, 1913, 376.

18. H. G. J. Moseley, *Phil. Mag.*, 26, 1024–34 (1913); 27, 703–713 (1914).

19. Bragg and Bragg, *Proc. Roy. Soc. A*, 88, 428 (1913), and 89, 246 (1913).

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21. G. W. C. Kaye, *Phil. Trans. Roy. Soc. A*, 209, 123 (1909).

22. F. Soddy, "The Radioelements and the Atomic Law" (Longmans, 1914), p. 41.

23. A. van der Broek, *Physik. Zeitsch.*, 14, 32 (1913).

24. Rutherford and Andrade, *Phil. Mag.*, 27, 854 (May, 1914), and 28, 263 (Aug., 1914).

25. A. Fleck, *Trans. Chem. Soc.*, 103, 381 and 1052 (1913).

26. A. S. Russell, *Chem. News*, 107, 49 (Jan. 31, 1913).

27. G. von Hevesey, *Physik. Zeitsch.*, 14, 49 (Jan. 15, 1913).

28. K. Fajans, *Physik. Zeitsch.*, 14, 131 and 136 (Feb. 15, 1913).

29. F. Soddy, *Chem. News*, 107, 97 (Feb. 28, 1913); *Jahrb. Radioakt.*, 10, 188 (1913).

30. N. Bohr, *Phil. Mag.*, 26, 496 (1913).

31. F. Soddy, "The Radioelements and the Atomic Law" (Longmans, 1914), p. 39.

32. E. Rutherford, *Phil. Mag.*, 27, 488–499 (Mar., 1914).

ALFRED D. COLE

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ADDRESS OF THE RETIRING VICE-PRESIDENT OF SECTION F OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

BEFORE proceeding to the special subject of this evening's address, which will be upon the research work of the Tortugas Laboratory of the Carnegie Institution of Washington, your retiring vice-president begs permission briefly to plead the cause of the Zoological Section of the American Association for the Advancement of Science.

Our grandfathers founded this association and during our fathers' day, in that tense period wherein the foundations of established beliefs seemed crumbling into chaos before the onslaught of Darwinism, the Zoological Section of the association was a vital force in bringing order out of the confusion of doubt and fear that beset the America of the seventies.

Then, in after years, there came the special societies, zoologists, anatomists, physiologists, ornithologists, entomologists and psychologists of America; and our Section F, having lost its appeal to the investigator as a clearing house for his ideas, has sadly languished.

However, let us not forget that the British Association which two generations ago was active in forming intelligent opinion in England, once also languished from a similar cause.

Then to our British cousins there came the light of a great idea. The field of their association expanded to embrace the whole imperial realm. Great meetings were held in Canada, South Africa and Australia, and the colonies became intellectually one with the mother country in a sense never known before.

The British Association is no longer a mere gathering of scientists, it is a mighty power in preserving that world-wide sympathy with ideals of democracy and fair play upon which the very existence of Britain's vast empire must depend. For England's strength is neither in acres nor in gold, but in the hearts of her sons who toil at many a stubborn task in many a distant land.

As servants of civilization, let the members of our own association meet the millions of America in a similar spirit.

At these meetings, let us speak with rather than to our countrymen.

Too often we may have looked upon the public as something colossal, crude and struggling, something far and apart from

our cloistered world within the college walls.

Let us come among our fellows not as doctors gowned and coped, but as the simple men and women that we are, seeking advice and aid more often than we can impart knowledge or develop wisdom. Realizing as we do that could we but exchange the known for the unknown, the little that science has achieved would appear contemptible. From the frontiers of our culture we gaze into the vast unknown, but it is but little that we can see.

Our science is not alone the concern of specialists but of every man and woman of our land and with the advent of modern medicine, antiseptic surgery, and a knowledge of the law of heredity, great human problems have arisen.

We now stand as trustees guarding things of vast import for good or evil. The very word eugenics conjures up problems for the wisdom of humanity to solve. These problems of science have shaped themselves from out the mists of doubt and lie as awful things upon our path, yet the higher the precipices the safer the harbor they enclose, and we await the wisdom of the wisest to guide us.

These are things too deep for the mere scientist, they are for each and every one of us, and the investigator is but one with the vast public in giving heed to their solution.

Yet in a deeper, more far-reaching sense, our association has a mission humanity-wide in its embrace, and as the duel has ceased to be respectable among individuals, so let war come to be regarded among the nations. It is with no boasting of virtue that we men of science of America can take this stand. We must speak as sinners pleading with sinners. Let us not forget that militarism has been in our own land as well as elsewhere. Let us remember that every generation of Americans has drawn the sword, and that the most prolonged and devasting conflict of the nineteenth century was waged on our own soil over a question which Great Britain solved through a simple act of Parliament.

The light of civilization has glorified the summit of our ideal but the vast mountain below has forever remained dark in the barbarism of the savage. Our new-born love of all humanity is superimposed upon ages of distrust, prejudice and hatred born of ignorance, but let us recognize that the spark of kindliness that seems so small to-day is ours at least to foster until true to its destiny it shines as a blessing to all future generations of our earth.

To effect these things what better body can there be than the men of science of the nations of the earth acting in cooperation with that vast multitude of our fellows from whom we have received the blessed opportunity to labor and to serve.

The problems of our fathers' day were trivial compared with these. Let us therefore be true to the old ideals of our American Association, and let it forever stand for *association* in terms of mutual helpfulness between our public and our men of science.[1]

Alfred G. Mayer

AID TO ASTRONOMICAL RESEARCH

The experience of the Rumford, Elizabeth Thompson and certain other research funds shows that great returns may be obtained from relatively small grants to suitable persons. Owing to the excellent organizations resulting from the large sums given to astrophysics in this country, astronomers are well qualified to secure such results. Accordingly, the follow-

[1] Dr. Mayer devoted the remainder of the evening to an account of the research work of the Tortugas Laboratory of the Carnegie Institution of Washington illustrated by colored lantern slides.

ing letter was sent, and the replies are given below in the order in which they were received.

My Dear Sir: The greatest return in astronomical output, for a given expenditure, in my opinion, could be obtained by moderate grants to leading astronomers. I am accordingly sending copies of this letter to twelve of the American astronomers who would, it seems to me, make the best use of a grant of one thousand dollars a year for five years. I want to publish these needs and then see if the money can be obtained. Are you inclined to make me a statement of about one hundred words showing how you would apply such a grant? I give below my own statement as an example.

Yours very truly,

EDWARD C. PICKERING

Statement

The New Draper Catalogue will fill eight quarto volumes of the Harvard Annals, and will give the class of spectrum and magnitude of two hundred thousand stars, or more. Miss Cannon has nearly completed the observations, but publication could be greatly expedited by the employment of an additional assistant at an annual salary of five hundred dollars. Another extensive research on the photographic brightness of the stars by Miss Leavitt, could be equally advanced in the same way. In carefully organized routine work a great increase in efficiency may be obtained by the use of such assistants.

Professor C. L. Doolittle desires $500 to $1,000 for publication of results already obtained.

Professor E. W. Brown approves of the determination of the position of the moon by photography as described below by Professor Russell, and suggests a determination of the lunar parallax by a similar method.

Professor F. Schlesinger states that a very efficient method for cataloguing stars by means of a photographic doublet has been developed at the Allegheny Observatory during the past few months. This is being applied to a zone four degrees wide at the celestial equator. We have been urged by astronomers here and abroad to extend this work to other parts of the sky. A grant of $1,000 per annum continued for about twelve years would enable us to cover nearly the whole northern sky in this way. Among other things, this work would help materially the progress of the great Astrographic Catalogue, and would increase its value. If the same work were done by the earlier and ordinary methods it would cost not less than ten times as much and would be far less accurate.

Professor S. A. Mitchell states that the Leander McCormick Observatory has undertaken as its principal work the photographic determination of stellar parallax, an important research to which the 26-inch telescope is excellently adapted. The income of the observatory for the payment of all salaries (except the director's), for maintenance, and for improvements is less than $1,500 per year. The award by Columbia University of the Adams Research Fellowship for the present year has made possible a much needed increase in staff. One thousand dollars per year would permit the continuation of parallax work, and would also allow expansion along visual lines and in photometric work.

Professor F. Slocum states that the Van Vleck Observatory will be finished during the summer of 1915. The principal instrument will be a new 18½-inch Clark refractor. The mounting and clock work will be made by Warner and Swasey. The observatory is to be used by classes in astronomy of Wesleyan University, but it is the intention of the director to devote as much time as possible to research. The chief feature of the proposed program will be systematic observations for the determination of stellar parallaxes. The midnight hours, when parallax factors are small, will probably be used for micrometric or photometric observations. An assistant at $1,000 per year to share in making the observations and to carry out the routine work of computation would greatly increase the efficiency of the observatory.

Professor E. B. Frost states that up to the end of 1913, 5,100 stellar spectrograms had been obtained with the Bruce spectrograph of the Yerkes Observatory for the purpose of determining the velocity in the line of sight of northern stars, chiefly of spectral types *B* and *A*, brighter than magnitude 5.5. Of

these spectrograms it has thus far been possible to measure only 2,740, or 54 per cent., owing to a lack of assistance in this work. For 1910–13 the percentage measured is 48. With an additional $1,000 per year for five years, additional assistants could be obtained so that these arrears of measurement could be made up and this program of work completed.

Professor H. N. Russell states that the photographic determination of the position of the moon at Princeton University Observatory, from plates taken at Harvard and measured here, has given results probably more accurate than any previous method of observation. The provision of a salary of five hundred dollars a year for a computer will enable the continuation of this work, which must otherwise be interrupted. An equal sum would provide a computer to work on eclipsing variable stars. Material for accurate light curves and elements of about one hundred of these systems, as yet uninvestigated, is contained in the Harvard photographs. The results regarding the density, surface brightness, and other characteristics of the stars would be of great astrophysical importance.

Professor J. A. Miller states that the energies of the observing staff of Sproul Observatory are largely devoted to research and stellar parallax work. "With such a grant as you propose at my disposal, I should employ two assistants, one at $500 per year to do the routine detailed work connected with a research of this sort; the second to aid in the measures and final reductions of the plates. I should have to pay the second assistant $800 per year, the difference between the $500 that you propose and the $800 to be paid by the observatory. I could thus materially increase the quantity of our parallax output without in any way affecting its quality. In addition, this would enable us to utilize our present equipment (without any additions whatever), more nearly to its full capacity, by extending our work into closely allied fields."

Professor J. Stebbins states that for the past few years the work of the University of Illinois Observatory has been the development of an electrical method for the measurement of the light of stars. "As the work is quite new, we must do a considerable amount of experimenting in the laboratory with the object of increasing the accuracy of observations at the telescope. These investigations are all carried on in addition to the regular instruction which must be done at a university observatory. We have some untrained student assistants, but if we could get the services of a regular man and keep him year after year, our scientific output would be greatly improved and increased. It is very probable that after a certain time the university will be able to put such a research position upon a permanent basis."

Professor G. C. Comstock states that the Washburn Observatory is engaged in determining the positions of several thousand stars, averaging about seventh magnitude, to be utilized in an extension of Boss's Preliminary General Catalogue. Progress of this work is greatly hindered by entire absence of a computing staff. One or more computers are sorely needed. A grant of $1,000 for a single year would be of value in this connection but such a grant continuing over five years would be much more than five times as useful since at the outset much time is necessarily given to training the computer to his work. His efficiency increases with experience.

Professor Philip Fox states that the most pressing need of the Dearborn Observatory is in the line of measurement of the many plates we have taken for the determination of stellar parallax, and the reduction of these measures. The series of plates now has reached 948, and is being added to at the rate of about ten plates on every clear evening. Additional help for this work would greatly expedite its progress.

Professor W. W. Campbell states that the greatest return which the Lick Observatory can make for a small additional expenditure would come from the employment of a very capable observer to assist with the spectrographs attached to the thirty-six-inch refractor and to the Crossley reflector. The demands

upon such an observer are very severe because the apparatus is extensive and complicated, and skilled and constant care must be devoted to the observations. Such services usually begin to be satisfactory about two months after the start is made, and their value increases through many years. The salary of such an assistant should be $1,000 for the first year and there should be an increase of $100 each year until $1,500 is reached.

An unexpected result was that in nearly every case, the principal need proved to be for assistants. Some preferred one experienced observer, others two computers. In some cases, it is believed that if the work were once started it would be continued by the university. An astronomer can often direct one or two assistants so that they will obtain as accurate results as if he devoted the same time to the work himself. A small appropriation may thus double the output of his observatory.

My own application is included since I believe that as important results can be obtained here as elsewhere, but if all can not be provided for, I recommend that other astronomers having fewer assistants should receive precedence. Even if only a portion of the sum asked for could be provided, it is probable that an extraordinary relative output would be obtained. It is hoped that, in some cases, those interested in a particular observatory may be willing to supply its needs.

EDWARD C. PICKERING

December 21, 1914

FRANCIS HUMPHREYS STORER

FRANCIS HUMPHREYS STORER, of the Massachusetts Institute of Technology and later of the Bussey Institution of Harvard University, was born March 27, 1832, and died July 30, 1914. His father was David Humphreys Storer (M.D., LL.D.) and his mother was Abby Jane (Brewer) Storer. He married Catherine A. Eliot, sister of Charles W. Eliot, June 21, 1871.

Professor Storer studied at the Lawrence Scientific School in 1850–51. He was assistant to Professor Cooke in 1853. He received from Harvard University the degree of S.B. in 1855 and the honorary degree of A.M. in 1870. From 1855 to 1857 he studied abroad and from 1857 to 1865 he practised as a chemist.

The writer knew him intimately from 1865 to 1870 when he was professor of general and industrial chemistry in the Massachusetts Institute of Technology and the writer was his pupil. In that year he resigned his position to become professor of agricultural chemistry in the Bussey Institution; the next year he became dean, an office which he held until he withdrew in 1907.

Professor Storer's pupils say of him that he was the best of teachers of chemistry. He and Professor Eliot were the pioneers in introducing the experimental method of giving instruction to classes in chemistry, and those who were ripe for it found in it the greatest inspiration. He was uniformly genial and had a great faculty of imparting his knowledge that was thoroughly interesting to his students. He possessed one of the most fascinating personalities of our day. Professor Rogers and Professor Storer were the two most inspiring teachers we had in the early days of Technology. Many of the pupils owe their absorbing interest in science to these two strong characters. In those days there were members of the faculty who were in favor of letting the students obtain the degree too easily; Storer was foremost in opposing this laxity, and insisted on the highest possible standard. He was a thorough teacher and a gentleman of high culture. He had a human side too, which endeared him to his pupils. On one occasion when an expedition was arranged to visit the coal mine in Rhode Island, all the party had arrived at the train and were excited and anxious because Eli Forbes had not turned up. At the last minute he appeared and stepped aboard as the train started and Storer remarked "and here is Eli the most prompt of us all."

All agree that his loss to Technology in 1870 was a severe one to that school.

Of his connection with the Bussey Institution it may be said he was always very ready

to help in any investigation that looked to the better condition of growing crops or of improving the land. He covered a wide range in those investigations and was sometimes criticized for matters that were not understood or seemed of small importance to ordinary interests. He was an excellent teacher and interested his pupils in the subject at hand, sometimes pretty dry, and did everything possible to give those matters a practical turn. His methods were clear and concise and he had little sympathy with slack work.

He was observant of current events and always spoke his mind freely in comment. He never "played to the galleries" but expressed his opinion of affairs as they appeared to him.

The Bussey Institution never had much money to carry on its work. Professor Storer was thus hampered by lack of funds and he was unfortunate in not having the faculty for getting help of this kind. He drew freely from his own funds, which could not have been over large, to help the school and the individual students.

In social intercourse he was never forward at all. His wife was a great help to him in this way, and they together did many kind acts to a newly appointed instructor at the Bussey Institution. On her death he drew into himself more than ever and had little intercourse in an every-day way with others.

Professor Storer's work is of the greatest importance in agricultural chemistry; in a way it is the foundation of modern agricultural chemistry. When he began, it was all new and he made the beginning.

Robert H. Richards

Boston, Mass.

THE ANTWERP ZOOLOGICAL GARDEN

From the date of the bombardment of Antwerp, apprehension has been felt regarding the fate of the beautiful and costly zoological garden of that city. Messrs. Lorenz and Heinrich Hagenbeck, both of whom are yet in Hamburg, alive and well, have furnished the *Bulletin* of the Zoological Society of New York with a copy of a letter received by them from Dr. Buttikofer, director of the Rotterdam Zoological Garden, which reads as follows:

All the bears in the Antwerp Zoological Garden were shot prior to the bombardment. The large feline carnivora were put into strong transportation cages and removed to the rear of the garden, likewise prior to the bombardment, while the small felinæ were transferred to cages in the cellars of the Festival Building. A few days before the surrender of the city, when the heavy cannonading started fires in all parts of the city, which could no longer be put out in consequence of lack of water, the large carnivora were likewise shot by resolution of the board of directors, *adopted contrary to the director's advice.* None of the other animals were killed, with the exception of a few venomous snakes. During the bombardment only one shell dropped into the garden, striking the ground in the open space for the turtles, where it fortunately did no material damage. Mr. L'hoest and his two younger children were my guests from October 5 to the earlier part of November, while the other members of his family likewise came to Rotterdam towards the end of the bombardment. Mr. L'hoest himself, whose mind had suffered severely from the effects of the terrible excitement and of the successive events which overpowered him, also came to Rotterdam for a few days, after the bombardment.

By the earlier part of November all the members of the family had returned to Antwerp.

The garden and the animals kept there have suffered no further damage during the seige, but you will readily understand that the number of visitors has so decreased as to be practically nil, while the membership will undoubtedly be reduced to such an extent that the very existence of the garden will apparently be put into serious jeopardy.

Everything here is in good shape, although there has likewise been a large decrease in our receipts, which compels us to be exceedingly economical. I presume that similar conditions prevail in all the zoological gardens in Germany, as well as in your country.

BENJAMIN PEIRCE INSTRUCTORSHIPS IN MATHEMATICS

The Division of Mathematics in Harvard University announces that hereafter on or about the first day of March in each year it will recommend two persons for appointment to Benjamin Peirce Instructorships in accord-

ance with the following regulations recently adopted by the president and fellows of Harvard College, provided suitable candidates make application: (1) Appointments shall be made by the president and fellows on recommendation of the division of mathematics. Each appointment shall be for one academic year and shall carry a remuneration of not less than $1,000 nor more than $1,200. (2) A holder of an instructorship may be a candidate for reappointment, but no person shall hold an instructorship for more than three years. (3) Each instructor will be expected to teach two and one half elementary courses and one other course which would ordinarily be of an advanced character. (4) Instructors will be permitted to attend without charge all courses of instruction under the faculty of arts and sciences, and to enjoy the same library privileges as other instructors. They will be offered every facility towards the prosecution of original scholarly work, the members of the division being ready to give all possible aid and encouragement. (5) A candidate for an instructorship in any academic year must present his name to the chairman of the division on or before the first of February of the previous academic year. He should offer at the same time evidence of his capacity as a scholar and a teacher. For this purpose he should present such documents as: (*a*) a dissertation accepted towards the fulfilment of the requirements for the degree of doctor of philosophy; (*b*) published contributions to mathematical science; (*c*) certificates as to his ability and success as a teacher; (*d*) personal letters relating to his character and qualifications for the post.

In amplification of the above regulations it may be pointed out that these newly-established Benjamin Peirce Instructorships afford an unusual opportunity for young men of good training and ability at or near the beginning of their teaching career. The appointments are made on the basis of an open competition; Harvard has one of the best mathematical libraries in the country; the amount of work required is very moderate (the "course" at Harvard being three fifty-five-minute periods a week throughout the year) and includes opportunity for advanced teaching. This advanced instruction will be on subjects selected in consultation with the instructor and, so far as possible, in conformity with his wishes. By exception applications will be accepted in 1915 as late as February 15. For further information address Professor Maxime Bôcher, chairman of the division of mathematics, 48 Buckingham Street, Cambridge, Mass.

COMMERCIAL GEOGRAPHY AND WORLD POLITICS

DR. ARTHUR DIX contributes to the *Geographische Zeitschrift* for June 11 an article with this title indicating a German point of view prior to the outbreak of the war. As summarized in the *Geographical Journal* he states that the tendency for inland communities to seek an outlet to the sea is becoming so marked that, with rare exceptions, such powers must now be regarded as in a state of unstable equilibrium. Such states in the general case must, in his opinion, either submit to be absorbed by the neighboring power which blocks the road to the sea, or must forcibly seize a stretch of seaboard. He gives Asia as an example of a continent in which independent inland states have now practically ceased to exist, Africa as one in which they are gradually disappearing. The Transvaal and the Orange Free State are given as examples of countries which, as soon as they acquired extensive relations with the world market, fell as booty to a neighboring power. As examples of two inland states which must in the immediate future undergo political change he gives Abyssinia, which he regards as being probably destined to fall into the hands of Britain, and Servia, which must, he thinks, fight for her free access either to the Adriatic or the Aegean. Secondly, great powers which have already one outlet to the sea tend to seek a double access. France has of course this double access already, but the founding of the triple alliance, the recent history of the Russian Empire, the opening of the Panama Canal, some of the difficulties among the various powers in Africa, the troubles in the Balkan

peninsula, are all discussed from the point of view of this search for an additional or an easier access to another sea. Thirdly, maritime powers, in his opinion, necessarily strive to extend their dominion over the coasts which face their own. Rome and Carthage, Italy looking across the narrow Adriatic, and also across the Mediterranean to Tripoli, France and Algiers, the designs of Britain upon the coasts which encircle the Indian Ocean, are all given as examples. Again he points out that when any power possesses a part of a navigable stream there is a tendency for it to seek to extend its dominion down to the mouth. Similarly, a colonizing power which has taken possession of the mouth of a river tends also to follow that river up to its source. The same thing may tend to happen in civilized countries, if the water of the river is used for irrigation, or if stream control is necessary. Thus the control of the lower course of the Vistula by Prussia is difficult because its upper waters are extra-Prussian. But the difficulty of the Polish question makes it necessary for Prussia to avoid covetousness in this connection, while a frontier adjustment which would deprive Prussia of the lower Vistula would cut off wholly German territory from the empire. The Rhine, he states, is another case where purely political conditions stand in the way of a natural economic tendency. It is an advantage to Germany for the mouth of this river to remain in the hands of a neutral state so long as the neutrality of this state is effectively maintained, for as it faces a powerful sea power, it would, if German, be liable to blockade in war time. Again, the fact that the Elbe and Danube are both Austrian as well as German rivers means that those two powers must either be allies or enemies, and these rivers thus form part of the geographical justification of the triple alliance. On the other hand, the relation of the great rivers of South America to the different states there suggests to the author that the political division of South America is in an unfinished condition, and that great readjustment will probably take place there.

Finally Dr. Dix is of opinion that a specifically modern cause of political differences among nations lies in warring interests in the construction of great transcontinental railway routes. The permanent tension between Britain and Germany he ascribes, not to the causes usually given on either side, but to the great extra-European railway schemes of the two powers. Germany, he says, is desirous of constructing and controlling an east to west line across the continent of Africa, while Britain desires to complete the Cape to Cairo route, to which Germany is strongly opposed. Similarly, he states that Britain is desirous of linking the Nile to the Indus by rail, and therefore opposes the completion of the Bagdad line to the Persian Gulf under German auspices. These causes of dissension might be got over by a mutual arrangement between the powers, or by a German-British alliance.

THE HUXLEY LECTURE

The Huxley lecture at Charing Cross Hospital was delivered by Sir Ronald Ross on November 2. From the report in the *British Medical Journal* we learn that before proceeding to the main subject of his address, which discussed recent advances in science and their bearing on medicine and surgery, with special reference to malaria and the transmission of diseases, he paid a well-conceived tribute to Huxley, who, Sir Ronald Ross said, was not only the bulldog of Darwin, and the interpreter of Darwin's profundities to the world, but also a patient and passionate investigator and a patient and dispassionate thinker regarding phenomena. But, the lecturer continued, Huxley was still more: he was a philosopher possessing all the very first qualities required for true philosophy. The clarity of his style was itself a guarantee of the genuineness and completeness of his thought. Secondly, his mind was fiercely critical in its search for truth, and he accepted nothing as fact which he himself had not endeavored to probe to the depths. Thirdly, no one has ever doubted that his aim was, not to astonish or to defeat or to persuade, so much as to reach the actual truth of every matter

with which he dealt. He would have been delighted, had he lived, to recognize the bearing of recent advances in science on the medicine of the tropics. Sir Ronald Ross devoted the main part of his lecture to tracing the history of the modern application of parasitology to etiology and pathology, dealing in particular with the growth of knowledge with regard to filaria, piroplasma and the malarial organisms. He sketched his own researches, the full history of which had, he said, been given in his Nobel lecture. It was only after several years' work that he recognized that the malarial infection was carried by a certain genus of mosquitoes only, not by *Culex* or *Stegomyia*, but by the anophelines. He made the observation that the spores of the analogous malarial parasites of birds which he had already recognized in *Culex* entered the insect's salivary or poison glands. This led to the disclosure of the full truth, namely, that the parasites were not only taken from man by the mosquitoes, as Manson had supposed, and not only put into man by the mosquitoes, as King supposed, but that both hypotheses were true, the insects carrying the parasites directly from man to man. Summarizing the results obtained, he said that from the time of the Romans it was known that the malarial fevers were connected with marshes and stagnant water in warm countries. Later, when it was seen that the disease was not confined to the proximity of marshes, the theorists conceived that there was a telluric poison which caused malaria and was especially abundant in damp places. All this was a very general proposition; and in order to prevent the disease, it was necessary to undertake very extensive drainage. The new knowledge obtained rendered it possible to particularize the exact route of infection. It was now known that the poison was not spread uniformly in the air of warm countries, but was always contained in the minute bodies of certain insects, and more than that, in the still more minute salivary glands of these creatures. The discovery of the full life-cycle of the parasites made it possible not only to predicate the route of infection, but to determine exactly which species of mosquitoes were concerned, and to study the habits of the inculpated species. The genera *Culex* and *Stegomyia*, which it was found did not carry the malarial parasite, breed most commonly in artificial collections of water around houses, the anophelines breed principally in natural collections of water such as marshes, puddles, streamlets and the edges of lakes, ponds and rivers. These observations showed the way to other inquiries which cleared up the epidemiology of yellow fever, sleeping sickness, tick fever, plague and might possibly throw light on that of dengue, Mediterranean fever and measles.

SCIENTIFIC NOTES AND NEWS

AT the meeting of the American Society of Naturalists, held on December 31, 1914, Professor Hugo DeVries and Professor Wilhelm Roux were elected to honorary membership.

SIGNOR GUGLIELMO MARCONI has been appointed a member of the Italian senate by King Victor Emmanuel.

IT is one of the privileges of the Spanish Academia de Medicina that it is entitled to a seat in the senate. The member of the academy recently elected senator in this way is Dr. B. G. Alvarez, one of the editors of the *Pediatria Español*.

DR. CHARLES R. VAN HISE, president of the University of Wisconsin, has been elected president of the Utilities Bureau, established as an agency for municipalities in their dealings with public utility companies.

DR. RAYMOND PEARL, of the Maine Agricultural Experiment Station, has been elected a member of the editorial board of the *Journal of Experimental Zoology*.

THE American Institute of Mining Engineers, the American Electrochemical Society and the Mining and Metallurgical Society of America are giving a complimentary dinner on Friday, January 15, at the Hotel Plaza to Dr. Frederick Gardner Cottrell, in charge of the San Francisco laboratory of the Bureau of Mines, in recognition of his contributions to research. It is well known that Dr. Cottrell assigned the patents for his process of electro-

static precipitation of fumes to the research corporation, the receipts from licenses to be used for the furtherance of research. As a result of this gift the research corporation is now in a flourishing condition.

The new officers of the Society of American Bacteriologists are as follows:

President, D. H. Bergey, University of Pennsylvania, Philadelphia, Pa.

Vice-president, John Weinzirl, Seattle, Washington.

Secretary-Treasurer, A. Parker Hitchens, Glenolden, Pa.

Council, K. F. Kellerman, W. A. Stocking, R. E. Buchanan and H. J. Conn.

Delegate to the council of the A. A. A. S., M. J. Rosenau.

The council of the society has decided to hold the next annual meeting in Urbana, Illinois, on December 28, 29 and 30 of December, 1915. There will be a special meeting in the summer in San Francisco the date of which has not yet been fixed.

The Paleontological Society at its recent Philadelphia meeting elected officers as follows:

President, E. O. Ulrich, Washington, D. C.

Vice-presidents, J. C. Merriam, Berkeley, Cal.; Gilbert Van Ingen, Princeton, N. J.; F. H. Knowlton, Washington, D. C.

Treasurer, R. S. Lull, New Haven, Conn.

Secretary, R. S. Bassler, Washington, D. C.

Editor, C. R. Eastman, New York.

Dr. E. F. Bashford has resigned the post of general superintendent of the Imperial Cancer Research Fund, which he has held for the past eight years.

Professor W. H. Kavanaugh, head of the experimental department of the college of engineering of the University of Minnesota, has been elected chairman of the Minnesota Section of the American Society of Mechanical Engineering.

F. C. Dosé has been appointed assistant in animal nutrition in the Pennsylvania Institute of Animal Nutrition.

Dr. E. G. Fearnsides, Miss F. M. G. Micklethwait and Dr. E. P. Poulton have been elected to Beit Memorial Fellowships for Medical Research.

Dr. J. Walter Fewkes, ethnologist of the Bureau of American Ethnology, left Washington on January 6, to take up his fieldwork in Arizona. Incidentally he will represent the Smithsonian Institution at the inaugural ceremonies of Rufus Bernhard von Kleinsmid as president of the University of Arizona. The exercises occur on January 11 and 12, at Tucson, Arizona. At the conclusion of the ceremonies, Dr. Fewkes will continue his archeological work in that state, and then proceed to New Mexico to conduct researches concerning the early inhabitants of the lower Mimbres Valley, in connection with which the institution has recently published a report.

A successful exploration tour through the wilds of Patagonia has been made by Dr. L. S. Rowe, of the University of Pennsylvania. Accompanied by the director of national territories and the governor of the Territory of Neuquen, Dr. Rowe traversed the southern section of the Argentine from the Atlantic to the Andes, and from parallel 37 to parallel 42 south. Dr. Rowe depicts in glowing colors the agricultural and industrial possibilities of this district, at one time looked upon as a desert region.

A cablegram from Cairo, Egypt, has been received at the University of Pennsylvania announcing the arrival of Dr. Clarence S. Fisher, leader of the university museum's Eckley B. Coxe, Jr., expedition, to make further excavations in Egypt.

Professor M. A. Rosanoff, of the Mellon Institute and the Graduate School, University of Pittsburgh, lectured on January 4 and 5 before the New York University department of chemistry, on the partial vapor pressures of mixtures in their bearings on the theory of solutions and the theory of distillation.

Dr. G. N. Stewart, of Western Reserve Medical School, lectured before the Syracuse Chapter of Sigma Xi on December 15, taking as his subject "The Physiologist in the Hospital and in the State." Other speakers this semester have been Dr. F. P. Knowlton, of the Syracuse Medical College, who spoke on

November 10 on "Hunger and Allied Phenomena" and Professor Henry A. Peck, of the Liberal Arts College, whose subject on November 17 was "Some Recent Aspects of the Nature and Extent of the Siderial Universe."

MR. CHARLES HALLOCK, the veteran author and naturalist, founder of *Forest and Stream* and the International Association for the Protection of Game, is now living at the John Dickson Home in Washington. He is full of interesting reminiscences. He tells the story that he first met the late Dr. Elliott Coues at the Smithsonian Institution when Coues was seventeen years old and took him with him on an expedition to Labrador in 1860. They made large collections and placed 164 skins in the late E. D. Cope's museum at Philadelphia. Mr. Hallock would be glad to hear from any of his old friends.

AT the annual meeting of the Mathematical Association, January 9, the president, Sir George Greenhill, gave an address on mathematics in artillery science.

MR. HILAIRE BELLOC gave the presidential address at the annual meeting of the Geographical Association, held on January 7, at the University of London.

A MEETING in commemoration of the four hundredth birthday anniversary of Vesalius was held at the New York Academy of Medicine, January 7, at which Dr. William H. Welch, of the Johns Hopkins University, delivered an address on "Vesalius and the Spirit of the Time"; Dr. Fielding H. Garrison, editor of the *Index Medicus,* spoke on "Anatomical Illustration Before and After Vesalius," and Dr. Harvey Cushing, of the Harvard Medical School, delivered a lecture on "The Portraits of Vesalius," illustrated by lantern slides.

PROFESSOR N. C. DUNÉR, the distinguished Swedish astronomer, secretary of the Swedish Royal Society of Science, died on November 10, at the age of seventy-five years.

THE U. S. Civil Service Commission announces an examination on February 2, for engineer of mine-safety investigations in the Bureau of Mines, Department of the Interior, at New York, N. Y., at a salary ranging from $3,000 to $3,600 a year. It also announces an examination for specialist in cotton testing in the Office of Markets and Rural Organization, Department of Agriculture, Washington, D. C., at salaries ranging from $1,800 to $3,000 a year.

THE Rockefeller Institute for Medical Research will receive $200,000 under the will of Henry Rutherford, for cancer research work. This bequest was threatened by a contest of the will filed by a cousin of the testator, but the contest was dismissed and the will admitted to probate.

A VIENNA medical journal, as quoted in the *Journal* of the American Medical Association, reports 844 cases of cholera with 331 deaths in Austria during the week ended November 7, including 90 cases in Vienna with 10 deaths. During the following week there were 78 cases in Vienna with 19 deaths.

THE international service of astronomical telegrams from Kiel having been interrupted by the war, the management of the "Zentralstelle" has been passed over to Professor Elis Strömgren, Copenhagen Observatory.

THE Entomological Society of France has resumed the publication of its *Bulletin.* No. 15 of 1914 has just reached this country. At the meeting of October 14, the president, M. Alluaud, in addressing the society stated that in the absence of both of the secretaries at the front the publication of Nos. 13 and 14 had been delayed. He urged members still remaining in Paris not to interrupt the regular meetings under any pretext and to attend regularly. He quoted the speech made by Doctor Laboulbène at the meeting of January 11, 1871, during the siege of Paris, in which the same course was urged. He further stated that a bomb had been dropped by an aeroplane very near the Museum of Natural History the day before the meeting. He stated that MM. Reymond Morgon and André Vuillet had fallen in battle. Vuillet was well known in this country on account of his publications relating to the gipsy moth and its parasites.

He was killed in an assault on September 10 at Ippecourt. Both of the secretaries, Dr. R. Jeannel and Dr. Maurice Royer, were at the front but in good health.

We learn from *The Observatory* that it is proposed to recommence the publication of the *Gazette Astronomique,* formerly issued by the Astronomical Society of Antwerp. The occupation of that town by the Germans occasioned the temporary suspension of the *Gazette.* A number of English astronomers, on the initiative of Mrs. Fiammetta Wilson, of Bexley Heath, have promised to aid by pecuniary assistance and literary contributions. It is intended to issue the *Gazette* monthly unless funds and the material for printing should justify publication at shorter intervals. The minimum subscription will be five shillings, but those who are able and willing to aid more generously may subscribe half a guinea or a guinea as a more effective means of carrying out the idea of their Belgian comrades. The *Gazette* will be published in French and English, and will be devoted to general astronomical subjects. It is hoped that the first number of the new issue will be ready early in January. Subscriptions and correspondence should be addressed to Felix de Roy, hon. sec., 29 Stamford St., London, S.E.

Dr. George D. Hubbard, head of the department of geology at Oberlin College, has issued a bulletin covering the work of the Oberlin Geological Survey during the summer. From June 25 to August 14, Dr. Hubbard conducted a field survey in geology at Glen Lyn, Virginia. The party consisted of Professor and Mrs. Hubbard and nine students, eight men and one woman. Three of the students were graduates; seven were Oberlin men; one came from the University of Cincinnati, and one from Houghton Seminary, New York. The work consisted of the making of a topographic map by means of transit and plane-table, and the making of a geologic map of about 40 square miles of hilly country, partly in the folded strata of the Appalachians and partly in the dissected Allegheny plateau of nearly horizontal strata. A large collection of rocks and fossils was made; among the latter were several species of crinoids hitherto unknown in the Mississippian rocks of this locality. These collections are now being prepared and mounted for the Oberlin Geological Museum.

With a view to prolonging the original survey of the Palestine Exploration Fund to the Egyptian frontier, and so connecting it with recent work of the Egyptian Survey Department, a survey has been carried out under the auspices of the same fund by parties headed by Captain S. F. Newcombe, R.E. In a report, written at Akaba in February of this year and printed in the July number of the *Quarterly Statement* of the Fund, of which we learn from the Scottish *Geographical Journal,* Captain Newcombe describes the general program of the survey, and the progress of the work to date, while a postscript written in June records the return of the party to this country after completing its labors. The area in question was but imperfectly known, and the results are of some interest apart from their purely cartographical value. Five parties were engaged in the work, and the map was made on the scale of 1:125,000, roughly contoured at 100-feet intervals. A triangulation was effected, the fixed points of the Siani boundary determined by the Egyptian survey being carried further east, while in the north a chain of triangles was carried across to connect with the original Palestine survey. In the south it was possible to save time and expense by observing large triangles, the exterior points—Mount Hor and Jebel Taba—being observed from two mutually visible fixed points over 40 miles apart on the Egyptian frontier, and a framework thus supplied into which subsidiary points could be easily fitted. The main difficulties encountered were concerned with transport, and the procuring of guides and supplies, with a certain amount of hostility on the part of the Arabs, but these were on the whole successfully overcome. A small area near Akaba had to be left unsurveyed owing to the unwillingness of the Turkish authorities to give the required permission. This was, however, mapped by Major Kitchener in 1883. The summit of Mount Hor

(where the size and bright coloring of "Aaron's tomb" were a distinct aid in the observation of rays over 55 miles in length) was fixed, relatively to the Siani survey, with a probable error of only 30 feet. The other results of the survey are mainly concerned with archeology, in the interests of which Messrs. Woolley and Lawrence, archeologists connected with the British Museum, were attached to the expedition for part of the time. Special care was devoted to the accurate collection of names of localities, which were taken down, as pronounced by the guides, by an educated Syrian of the party. That of "Theigat el-Amirin," thought by Professor Palmer to be possibly a relic of the Amorites, turned out to refer to a tribal fight of about 150 years ago between the Azazma and Amiri tribes. The Bedawin of this desert region seem to have moved there only within the last 500 years. All roads have been inserted in the new map, and a point of interest is the discovery that the direct route from Kadesh to Mount Hor is an easy road, though thought by earlier writers to be impossible.

THE *Royal Engineers' Journal* for May of this year contains, according to *The Geographical Journal,* an account by Captain C. W. Biggs, R.E., of a recent somewhat serious encroachment of the sea at Fort Ricasoli, Malta, and of the measures taken to cope with it. The paper is illustrated by plans and photographs. The site of the encroachment is a line of weakness, due apparently to a fault in the rock structure, traversing the peninsula on which Fort Ricasoli is placed, on the northeast of the Grand Harbor. The trace of the bastion walls built by the knights of Malta about 1670, shows that even then the inlet on the line of fault existed. The winter storms have gradually eaten into the fault and burrowed a tunnel beneath the cliff, from which a sort of chimney was formed leading to the parade grounds above. Measures were therefore necessary to stop the encroachment, the proposal adopted being one for the creation of a breakwater across the mouth of the inlet by means of a number of concrete blocks chained together. The work has been continued for several successive years, the comparatively small blocks used at first proving inadequate to withstand the heavy seas. Some were washed out to the front, while others sank in the sand. The weight of the blocks was progressively increased until at last it reached fifty tons each, and this seems to have had a satisfactory result. Although some of the blocks have been shifted by the winter storms, this has now taken place *inwards,* while the sea has helped to defeat itself by piling up material behind the breakwater. All that is thought necessary for the future is the addition of more large blocks from time to time, and continued filling in behind them.

IN the thirty-fifth annual report of the United States Geological Survey, Mr. Geo. Otis Smith, the director, discusses particularly the province of the federal survey. An amendment which was offered in Congress to last year's appropriation bill would, if passed, have restricted the geologic work of the survey to the public lands. As the amendment failed the only result was to attract more attention to the basic investigative work of the survey, which embraces all the lands of the United States, the privately owned as well as the public lands. The examination of private property for private purposes is forbidden by the organic act of the survey, but the examination of private lands must be included in any general investigation. The determinative factor in the whole matter is whether the investigative work on privately owned lands yields results that are merely of local and personal interest or results that are of general and national value. Land ownership is only an incident when large questions of natural resources are considered. The special interest of the government in its own lands—the public lands—being granted, it must be added, as was suggested last year by Representative Sherley at a hearing before the House Appropriations Committee, that "So far as the development of the mineral resources of the country is concerned, it is just as important to know the resources of privately owned land as of government-owned land." When it is remembered that in the First Annual Report

of the Geological Survey Director King prophesied for the United States a future annual output of mineral products having a value of a billion dollars, and that the present production is two and one half times that amount, it must be conceded that the desirability of the federal scientific investigations of these national resources is even greater than in 1880. "It is a most conservative statement," Director Smith says, "that at no date has the general public been in closer touch with the United States Geological Survey or made larger use of the published or unpublished results of its surveys and investigations than at the present time."

UNIVERSITY AND EDUCATIONAL NEWS

A bequest of $3,000,000 to Oberlin College by Charles M. Hall, the distinguished electrochemist and manufacturer of aluminum, is announced. The bequest is in the form of $2,000,000 endowment to be used for any purpose, $500,000 to be used to build an auditorium, $100,000 for the auditorium's maintenance, $200,000 to be spent for campus improvements; all property in Oberlin owned by Dr. Hall, and a valuable art collection.

The will of Miss Grace Hoadley Dodge, for many years known for her educational and philanthropic activities in New York City, contains bequests of $1,400,000 for educational and charitable purposes, as well as a number of deferred bequests of the same character. The sum of $500,000 is bequeathed to Teachers College, Columbia University, in the founding and conduct of which she took an active part. The college will receive two deferred bequests, one of which may be large. To the National Board of the Y. W. C. A. the sum of $500,000 is left, and to the Y. W. C. A. of New York City, $200,000.

At the meeting of the corporation of Harvard University on December 28, it was voted to establish a separate faculty for the Bussey Institution. The vote was consented to by the board of overseers, and the new body at present includes the following members: W. M. Wheeler, Ph.D.; W. E. Castle, Ph.D.; R. T. Fisher, A.B., M.F.; E. M. East, Ph.D.; C. T. Brues, S.M.; I. W. Bailey, A.B., M.F., and C. C. Little, S.D., of the Bussey Institution; G. H. Parker, S.D. and W. J. V. Osterhout, Ph.D., of the faculty of arts and sciences; and E. E. Tyzzer, A.M., M.D., of the medical school.

Dr. C. E. Burke, lately of the University of California, has been appointed instructor in the department of chemistry at the University of Vermont.

Dr. Howard D. Haskins, formerly associate professor of bio-chemistry in the school of medicine of Western Reserve University, has been appointed professor of bio-chemistry in the medical department of the University of Oregon.

Dr. Frederick D. Heald, of Philadelphia, has been appointed professor of plant pathology and pathologist, Washington State College and Experiment Station, Pullman, Washington.

DISCUSSION AND CORRESPONDENCE

BATESON'S ADDRESS, MENDELISM AND MUTATION

In Bateson's thoughtful and stimulating address,[1] a recognized authority on evolution attempts to summarize for us recent progress in the study of that subject by analytical methods. It would be well for all engaged in some particular branch of this subject to attempt thus to lift the eyes from the scene of their individual labors and survey from time to time the entire field. An indispensable sense of proportion and perspective is thus gained. This is my excuse for commenting briefly on some of Bateson's fruitful ideas.

That evolution occurs all biologists agree. That the organisms now existing on this earth are different from those which formerly existed here no one questions. But we are still ignorant of how they came to be different. The geological record indicates that the change was gradual. The supposed ancestors of the horse,

[1] Bateson, W., Address of the President of the British Association for the Advancement of Science, Science, N. S., 40, pp. 287–302, August 28, 1914.

for example, are less and less like modern horses the more remote in geological time are the deposits in which their bones are found. But students of evolution differ in their ideas as to how gradual the progress of evolution has been and is, for no one supposes the process ended.

Within my view stands the sloping bank of a reservoir, which most visitors ascend by a flight of granite steps; but children often go up the grassy bank wherever they happen to encounter it. Either method takes one to the top, the gradual or the stepwise mode of ascent.

Evolution was thought by Darwin to occur in two ways comparable with these, the gradual and the stepwise. From Darwin's writings it would seem that he regarded the gradual as the more common and important method of evolutionary change among organisms, but it is clear that he recognized stepwise or "sport" variation as of considerable value, particularly in the production of new varieties under domestication.

But not many years after Darwin's death a question arose in the minds of certain thoughtful naturalists as to whether Darwin had rightly estimated the relative importance of these two methods of evolution. Galton, Bateson and DeVries have laid increasing emphasis on sport variations or "mutations," until these have come to be regarded by many as of overshadowing importance in evolution. The full-fledged mutation theory[2] maintains that evolution occurs *by steps alone,* that is that new species arise from old ones by single discontinuous steps, never by gradual uninterrupted change. This theory has been the guiding principle in evolutionary study in recent years. Its basic idea is that natural species are invariably discontinuous and that intergrades between them do not occur except possibly as the result of sporadic hybridization, such intermediate forms being unstable and so without significance. The attempt by Bateson[3] to classify the variations which occur within species led him to the conclusion that only such variations as are discontinuous in nature can have species-forming value, since they alone are not "swamped by crossing." This idea has been supported by the observation that among species regularly dimorphic or polymorphic, the several forms which remain distinct, notwithstanding constant intercrossing, are Mendelian alternatives, conforming with the laws of dominance and segregation. Many of the striking variations in color and form which occur among domesticated animals and plants follow these same laws so that their rediscovery and verification in 1900 was rightly regarded as strong presumptive proof of discontinuity in evolution. At about the same time DeVries brought together in his book entitled "The Mutation-Theory" a large amount of evidence favoring the idea of discontinuity in evolution most important of which was the repeatedly verified polymorphism of the seedlings produced by Lamarck's evening-primrose.

Mendelian segregation, however, does not at present offer a sufficient explanation of mutation in the evening-primroses so that provisionally we are forced to conclude with Gates[4] that mutation and segregation following hybridization are probably distinct phenomena. It also remains doubtful whether the phenomena observed among evening-primroses occur at all commonly among other plants or among animals. The so-called "mutations" which Morgan has observed in the fly *Drosophila* are certainly not of this order, but are clearly due to Mendelian factorial variation. Many with Bateson think that Mendelism affords a basis for the explanation of all evolution and confidently expect the evening-primroses sooner or later to be shown conformable with its fundamental ideas. In the

[2] The term "mutation theory" is here used in its widest sense, including not merely the ideas of De Vries concerning evolution among the evening primroses, but the general idea of discontinuity in the origin of species previously outlined by Galton and Bateson.

[3] "Materials for the Study of Variation," 1894.

[4] Gates, R. R., "Breeding Experiments which Show that Hybridization and Mutation are Independent Phenomena," *Zeits. f. ind. Abst. u. Vererbungslehre,* 11, pp. 209–279, 1914.

latest statement of his views, Bateson argues substantially as follows: Variations may be large or small. Those which are small are either not inherited or are of no consequence, being "slight differences that systematists would disregard." But large differences can not arise "by accumulation of small differences." Hence only large differences have evolutionary significance. In his own words:[5]

> We have done with the idea that Darwin came latterly to favor, that large differences can arise by accumulation of small differences. Such small differences are often mere ephemeral effects of conditions of life, and as such are not transmissible; but even small differences, when truly genetic, are factorial like the larger ones, and there is not the slightest reason for supposing that they are capable of summation.

Whether we may properly regard small differences as capable of "summation" depends upon what we mean by summation. Phillips and I[6] have shown that in the case of piebald rats the areas of white fur characterizing the race may be either increased or decreased at will and that the change takes place gradually, progressing steadily generation after generation and far transgressing the original limits of variation. The same is undoubtedly true of similar variegated patterns which mendelize among both animals and plants. Small differences which have arisen spontaneously have certainly been *aggregated* in this case. But crossing of the modified races shows that the aggregated changes have not been summated to such an extent that they constitute a single Mendelian factor, except in one case, where it seems quite possible that something of this sort has occurred. I am by no means ready to regard summation out of the question, whether by that we mean mere aggregation or fusion into a new Mendelian unit.

Bateson has further expressed the view that evolution has occurred largely, if not exclusively, by loss of Mendelian factors resulting in striking variations that breed true from their first appearance and thus render the parent species dimorphic or polymorphic. That many varieties of domesticated animals and cultivated plants have originated in this way will be admitted by any one who has studied them genetically. Darwin himself pointed out the importance of "sport" variation in producing new varieties of animals and plants under man's care and supervision and it is known that similar variations occur in wild species. But it is doubtful whether in a wild species a sport originating in this way has ever replaced the original form. Under domestication it is only the constant interposition of man that keeps the favored sports alive. Whether sport variation has had any part in the evolution of species is accordingly very doubtful. If we compare one wild species with another, we commonly find existing between them not single striking differences but numberless minute differences. Systematists usually name as diagnostic characters a few of the more striking differences, ignoring, as they are quite warranted in doing, all minor ones, the enumeration of which is for their purpose superfluous. But if one makes an intensive study of related species he finds that they differ in endless details of structure and physiological behavior extending even to differences in size of the constituent cells of the body (Conklin), or of their parts (chromosomes, chromomeres, etc.). During recent years, as the discrimination of species has become more keen, it has taken on more and more a quantitative expression. Series of specimens are measured, and specific distinctions are based on absolute and relative dimensions of parts, not on the "presence or absence" of large striking features of organization. It is easy to dispose of the work of the systematist by assuming that he does not know his business, but is it wise to do so? For other lines of evidence also indicate that the differences between species are quantitative and increase with genealogical divergence. This for example is the conclusion reached from such distinct methods of study as the examination of the forms of hemoglobin crystals in the blood of various species of animals and of the precipitin reaction of the blood.

[5] *L. c.*, p. 285.

[6] Publ. No. 195, Carnegie Institution of Washington, 1914.

But quantitative differences such as distinguish species are precisely those which do *not* Mendelize in crosses. Bateson says (p. 291):

> Of the descent of quantitative characters we still know practically nothing.

By which he probably means that we know *nothing Mendelian,* since in this address he treats Mendelism as the all-sufficient basis of evolution, and ignores a decade of intensive work in America directed toward the discovery of Mendelian factors as a basis for quantitative differences, a work participated in by many different workers, all favorably disposed toward the idea, but all unavailing. For the uniform result of a cross which involves quantitative differences is the production of an intermediate, which in turn produces intermediates only slightly more variable than the races originally crossed. Dimorphism or sharply discontinuous polymorphism is regularly wanting after size crosses. This is a fatal objection to the idea that specific differences are discontinuous in origin. One who advocates this idea has no choice at present but to ignore (as Bateson does) all evidence derived from the experimental study of this subject.

The idea that large differences can not arise by summation of small ones is rendered improbable by this evidence. For if the larger (quantitative) differences can so readily be broken down into smaller ones, it seems highly probable that the process is reversible. Indeed the experience of breeders shows that it is. The dog-breeder alluded to by Bateson who titrated his colored fluids to illustrate blood dilution in crosses was, so far as quantitative characters are concerned, employing a very apt method, notwithstanding Bateson's disapproval of it.

Even sport variations, which truly Mendelize, and which form the basis of color varieties and other fancy varieties among animals and plants, even these are capable of secondary break-down or "fractionation," as Bateson admits. In making this admission he differs from the supporters of the pure-line theory who conceive that a Mendelian factor is incapable of change, but who apparently hold the idea as an article of faith rather than one requiring proof.

Secondary break-down or modification of Mendelian factors is, however, coming to be so generally recognized that a special name is now applied to its products, that of multiple allelomorphs. Even those who hold to the conception of "pure lines" now recognize that the same sport variation ("mutation" or "locus") may assume several different conditions which viewed quantitatively form a graded series; but they insist on the *discontinuity* of the grades or forms which a Mendelizing character may assume, maintaining on *a priori* grounds that these stages can not be bridged. The perilousness of such a position is apparent from a single well-known case. The first discovered Mendelizing character in animals was albinism and it is one of the simplest and clearest cut of all Mendelian characters thus far discovered. It was not to be expected that the single step between a wholly uncolored and the normally colored condition would be or could be bridged. Yet two such intermediate stages have already been demonstrated, which are unmistakable allelomorphs of albinism, *i. e.,* which behave as alternative forms of the same genetic factor. If two such intermediate stages may arise, why may not others arise; why not a dozen, why not a thousand? Is it safe to assume that this is not possible?

Bateson urges that in cases of color variation such as that of the sweet-pea and the primrose the *large* changes came first and the smaller ones later by secondary break-down or "fractionation." The argument implies, indeed, he expressly claims, that large differences can not be built up from small ones. I do not believe that either paleontology or the history of breeding will support Bateson in this claim. On the former ground Osborn[7] holds to a gradual origin of discontinuous differences between organisms. A study of breeds of animals in comparison with their wild originals or present-day representatives shows

[7] Osborn, H. F., "The Continuous Origin of Certain Unit-characters as Observed by a Paleontoogist," *Amer. Nat.,* 46, pp. 185–278, 1912.

that variation has not occurred merely by large losses subsequently fractionated so as to form intermediates. Not merely intermediates arise but also those which transcend any known original sports. Original black races have become blacker; original yellow races have become yellower; white-spotted races have become more spotted still at the will of the breeder. Large races also become larger, and small races smaller, under the hands of the fancier. He does not limit himself to the production of intermediates.

To suggest further that all variation transcending limits previously existing is due to loss of inhibitors and so is really retrogressive is scarcely satisfying. It is a formal evasion of the difficulty but in no sense a solution of it. It belongs with the box-within-box idea of development. I agree with Bateson that variety formation within the higher animals and plants seems to be very frequently by a process of loss but I can not believe that this is the exclusive process concerned in the formation of new species or even of varieties. It needs but to carry the idea to its logical conclusion to show its absurdity. Is man merely an amœba simplified by loss of inhibitors? I can not believe so. I can not believe that the original proteid molecule has since its original synthesis only grown simpler. New radicles have undoubtedly become attached to it as side-chains replacing or supplementing old ones and changing its properties. The living substance is not merely *losing* constituents; it is also gaining new ones. Similarly organisms, morphologically and physiologically, change not merely by losses but also by gains. It is impossible to explain evolution satisfactorily by either process alone. The two go hand-in-hand and no doubt are constantly occurring among organisms. Change is universal. Mere subdivision of a species into two groups of individuals, which are prevented from intermingling, seems to be sufficient in time to make the two groups specifically distinct. Each keeps on changing in so many different ways that it would be little short of a miracle if both changed similarly and simultaneously in all respects. Direct environmental effects are insufficient to account for such organic changes, for among the best-known illustrations of divergent evolution are the animals of oceanic islands, close together and subjected to the same climatic agencies, undoubtedly descended from common ancestors at no remote period, yet having become distinct, probably through numerous spontaneous changes which isolation prevented from being ground down to a common level by inter-crossing.

These are commonplaces of evolutionary knowledge, familiar to everyone since Darwin and Wallace first called attention to them, yet we are in danger of overlooking them for the moment in our enthusiasm over a new method of attacking the obstinate problems of evolution. It may not be superfluous therefore to call renewed attention to them in this connection. Spontaneous variation is still with us and is as widespread as it was in Darwin's time. It is doubtful whether unvarying "completely homozygous" organisms occur anywhere outside the text-books. In the case of organisms known to be varying genetically there is abundant evidence that small variations are heritable no less than large ones, and we are by no means "done with the idea" that small variations are capable of summation.

With Bateson we must deplore the necessity of engaging merely in destructive criticism. It is indeed "a low kind of work." It would be so much easier, pleasanter, and more satisfying to adopt a single explanatory principle for evolution and build on this. But it would be foolish to go on building lofty superstructures of hypothesis on an insecure foundation, and the more carefully we scrutinize the mutation theory the more serious do our doubts become whether it is a secure foundation for evolutionists to build on.

W. E. Castle

Bussey Institution,
Forest Hills, Mass.,
December 12, 1914

Mastodon Tusk in Glacial Gravels

To the Editor of Science: A tusk of a proboscidean, probably *Mastodon americanus,* was

found recently in a gravel pit in Pony Hollow, twelve miles southwest of Ithaca, N. Y., on the property of Mr. Bert Drake. Unlike most *Mastodon* finds from this region this is not postglacial. It was found in place twenty-four feet below the surface in stratified sand and gravel which was being used in good roads work. The pit is in the base of an extensive terrace whose top follows the valley wall high above the outwash gravel plain which occupies the floor of the valley. The exact origin of this Pleistocene terrace is obscure but it is certainly not later than the end of the ice occupation of the valley and may be earlier.

The tusk was broken in removing the gravel. Two pieces, each about a foot long, from ten to thirteen inches in circumference, were presented to the Paleontological Museum of Cornell by E. A. Dahmen, the road engineer. Three approximate measurements of the curvature of the tusk gave from two feet one inch to two feet eleven inches as the radius of curvature.

PEARL SHELDON

CORNELL UNIVERSITY

SCIENTIFIC BOOKS

THE TRANSLOCATION OF MATERIAL IN DYING LEAVES [1]

THE fact of an autumnal transfer of nutrient matter from leaves was first clearly stated by Sachs, in 1863. Sachs's statement was based on microscopical examinations of the leaves of a series of plants in various stages of their autumnal changes, whereby he determined that starch and chlorophyll disappeared from leaves before their fall. He extended this observation to cover most of the other nutrient materials in the leaf. Swart, however, holds that the solution of this question in its broader sense is to be had only by chemical analyses such as he has made.

According to Swart, the first essential to a correct answer is a correct wording of the problem, as follows:

> During the autumnal coloring of leaves can there be determined, by chemical analysis, a translocation of nutrient materials during the period extending from shortly before to directly after the close of the yellowing?

It is essential to draw the time limits thus sharply, since this period represents a sharply limited, externally recognizable terminal period in the life of the leaf, which in addition to anatomical variations in the petiole, may be directly recognized by the physiological processes which are indicated to us by the disappearance of the chlorophyll. If we would answer the question as to whether, before the fall of the leaf, there may be demonstrated a transfer of any substances, it is necessary to regard only the period during which the leaf, as indicated by these externally recognizable processes, prepares for its final act. After answering this narrower question, then we should consider the amount of these materials in the leaf at other periods of the year, in order to arrive at a causal explanation of the phenomena.

The necessary chemical analyses fall into two groups: those which extend over the entire vegetative period of the leaves, and those which cover only the period directly before and after yellowing. In most of the former investigations the leaves analyzed began to be removed from the tree at a late period, but quite independent of the exact time when the coloring began and ended. The value of the results of such analyses must, therefore, be estimated with caution since, even in most favorable cases, they can only give us an answer to the question of whether, in general, yellow leaves are poorer in their content of any given substance than green leaves of any earlier period. Thus a maximum or a minimum in the proportion of the given material in the intervening period would give results entirely misleading with reference to the question whose answer is sought.

Another objection to former researches that extend over the entire vegetative period is that very frequently a large number of leaves for study were taken at one time from the same

[1] Swart, Dr. Nicolas, "Die Stoffwanderung in ablebenden Blättern," pp. 1–118, Taf. 5, Jena, Verlag von Gustav Fischer, 1914.

tree. By this act the metabolism of the remaining leaves is known to be altered. Thus, for example, the rapidity of the translocation of carbohydrates from leaves is increased with the diminution of the number of leaves on the plant; by partial defoliation transpiration from the remaining leaf surfaces is increased, and this is accompanied by a more rapid depletion of starch. The taking in of the nutrient salts is also favored by a diminution in the number of leaves.

If dead leaves are used in the analysis, another source of error is introduced, since substances may be washed out of dead leaves exposed to the weather. In former researches a yellow color has been held to indicate death, and all yellow leaves accordingly designated as dead. Swart discusses these, and other sources of error in all researches on the subject prior to Wehmer's *Kritik* of 1892. He argues with Wehmer that all results published before his paper can be relied upon to only a limited extent, but he disagrees (p. 37) with Wehmer's view that these results argue against the validity of the theory of a depletion before leaf-fall, for he knows of no case where the failure of a translocation has been demonstrated beyond doubt.

In his own researches Swart employed the method devised by Stahl, of cutting out, with a die (or a cork-borer), portions of the leaves for analysis, thus avoiding the troublesome and inaccurate measuring of the area of an entire leaf. Tabular statements of the results of several analyses are given on pages 59 to 67, the material including leaves of *Anthurium*, sp., *Liriodendron Tulipifera, Ginko biloba, Laserpitium latifolium, Parothia persica, Æschylus Hippocastanum, Salix capræa, Sorbus intermedia, Quercus* sp., and others. These analyses show that the leaves in the brief period preceding their fall and during their coloring, had suffered a loss of nitrogen, phosphoric acid and potassium, and these losses, especially in the case of nitrogen and phosphoric acid, were quite considerable. These results confirm *in toto* those of former investigators. The slight loss of magnesia in individual cases may, in the opinion of the author, be attributed to variations in the analyses, in consequence of the inequality of the research material. Iron remains rather constant.

It is worthy of note that calcium, silica, phosphoric acid and chlorine, materials which at other times become richer in leaves, increase only a little, or not at all. From this it may be inferred that, in general, in the last vital period of leaves, only slight additional quantities of nutrient salts are taken in from the soil. On the other hand, the two analyses of *Laserpitium* (as shown in the table, page 67), in so far as they are comparable, indicate that the composition of leaves may vary considerably in different years.

No general law or principle can be formulated from the known behavior of nitrogen, phosphoric acid and calcium. Such an inference must be based on a study of all the investigations bearing on the subject. The fact that the literature contains certain contradictions as to the behavior of calcium, serves only to indicate (in so far as the researches have been carried out with sufficient care) that the behavior of any given substance is not uniform, but varies, within wide limits, according to the plant and the external conditions.

In consideration of the rather considerable losses, especially of nitrogen and phosphoric acid, suffered by the leaves studied by Swart, he finds himself in entire agreement with the view for which Ramann and Stahl contend, namely, that the loss of substances suffered by leaves before their fall is wholly restricted to their period of yellowing. Microscopic investigations of yellowed leaves (pages 70–96) also substantiate this conclusion.

The second chapter is devoted to The Coloring of Leaves. Having demonstrated that the translocation of nutrient materials, resulting in a permanent loss of these substances from the leaves, takes place in the relatively short period of a few weeks' duration, marked externally by the loss of chlorophyll, the author now turns his attention to this period, in order to ascertain how far a study of the yellowing of leaves would lead to the same conclusions as did the chemical analyses.

The period of yellowing or of coloring in

general, in which are completed those processes which are associated with the degeneration of the leaves, varies in length (according to the species of plant) from a few days to several months. In unusually dry, or in wet cold years (*e. g.*, 1911 and 1912 in Germany), this period began several weeks earlier than normally, and was of briefer duration. Under these conditions the loss of substance which leaves suffer through autumnal translocation is notably less than under normal conditions, especially when an early night frost brings the life of the leaf to an abrupt end.

That the vascular bundles exert an influence upon the process of change of color is evidenced by the fact that the change always begins in those parts of the parenchyma that are farthest from the veins, and spreads thence by degrees, first to the smaller veins, then to the main veins, reaching the petiole last of all. As the yellow color spreads the stomata (as Stahl has shown) quite generally become closed.

Swart then asks what has become of the chlorophyll, where the yellow color has appeared, and notes that this question can not be considered to advantage, except in the light of the newer conceptions concerning the nature of chlorophyll. Then follows a review of the chlorophyll studies of Sorby, Tswett, Willstätter, Kohl, Czapek and others. According to Kohl (1902), there belong to the yellow pigments of chlorophyll, carotin and two xanthophylls, α and β. Tswett (1903) distinguished in addition to carotin, three xanthophylls, α, α_1, β. From subsequent researches of Willstätter we have become better acquainted with two of these yellow accompaniments of chlorophyll. One is an unsatisfied carbohydrate, of the composition $C_{40}H_{56}$, identical with the carotin of carrots. The second, the xanthophyll of older authors, is an oxide of carotin, with the formula $C_{40}H_{56}O_2$. Both pigments are crystallizable. According to Willstätter, both these yellow pigments play no rôle in photosynthesis, but are concerned in respiration, and with this view Swart agrees.

Returning now to the question as to the behavior of these green and yellow pigments, during the yellowing of the leaves, Swart rejects the hypothesis that the green ones disappear from the leaves, while the yellow ones, as such, remain behind. As to whether the chlorophyll is merely translocated as such, or whether it first undergoes a decomposition, there is no *a priori* reason for considering the first alternative as correct, while at the same time no instance is known where the colorless rhizomes of our perennial plants become green in winter. Observations of the bark of girdled branches above the girdling, as compared with the bark below the girdling, also give negative results, and so on this ground also, we may reject the hypothesis of a translocation of chlorophyll as such.

If, however, the chlorophyll suffers a disintegration, then either the disintegration products are removed from the leaf, or else they remain where formed. Stahl (1909) showed that the former is the case, and demonstrated that the separation of a leaf from the stem, or portion out of the leaf blade, caused a marked retardation of the yellowing in the leaf or isolated portion. The same result was obtained on trees in the open, in the case of leaves at the commencement of color change, by separating a part of the blade from direct connection with the vascular bundles, by a cut or simply by pinching. Swart has repeated these experiments with the same results as those obtained by Stahl. For their firmer establishment, there is needed only the application of the law of mass action. Swart, therefore, concludes, with Stahl, that the chlorophyll passes from the leaves into the stem in the form of its disintegration products.

The question of the fate of the yellow coloring matters is next considered. Kohl held that the yellow pigment that remains behind in yellowed leaves consists of carotin and also of the xanthophylls, α and β, found by him in green leaves, but that the yellow pigment of the normal green chloroplasts remains unaltered during the autumnal color changes. In direct opposition to this Tswett, by means of his adsorption method, found that, in most cases, the larger part of the yellow pigments in yellowed leaves represents a new formation,

which he provisionally designated as autumn-xanthophyll. The question as to whether it is simple or complex, Tswett did not solve. If Tswett's idea is correct, then it is highly probable that we have to do here with a derivative of the normal yellow pigment of the leaf—possibly with a further oxidation product of carotin. Swart refers to the contrast (first noted by Stahl) in the behavior of the yellow and green pigments, and the biological significance of yellowing and etiolation, showing how the yellow pigments containing the elements C, H, and O, remain in the leaf, while the green pigments, which contain also N and Mg (two elements whose retention by the plant is more important than that of the other three), are transplanted back into the plant before the death of the leaves.

Here is a lack of agreement with the results of the chemical analyses, which show that the magnesium is not removed from yellowing leaves, and a satisfactory explanation of this contradiction in favor of the results above recorded is not at hand. One may consider that the magnesium contained in chlorophyll represents only a very small part of the entire amount of that element which is present in leaves, so that, considering the variations which the results of the analyses always show, the translocation of this small part may be often not noticeable in the end result. Unless we abandon the idea of a translocation of the chlorophyll in the form of its disintegration products, we must establish the fact that the magnesium very probably takes no part in this movement.

Swart then gives attention to the changes in the cell contents of the yellowing leaves disclosed by a microscopical examination. Four different processes (to which Sachs first called attention) are studied: (1) The destruction of the outer form of the chlorophyll grains, (2) the disintegration of the green pigment, (3) the translocation of the chlorophyll substance, (4) the translocation of the starch. These processes, which may occur in any order or simultaneously, are studied and described. Contrary to Mer, Swart believes that the oil, formed by the disintegration of the chlorophyll, remains in the leaf at the time of leaf fall. Wide variations are noted in the behavior of the starch. In some leaves which have just begun to turn yellow, very little starch was found, while in others, quite yellow, the yellow portions were found to be full of starch. Swart confirms the observations of Mer as to the sequence of steps in the disappearance of the starch, namely, first from the parenchyma cells of the leaf blade, then from the veins of the blade, and finally from the vascular bundles of the petiole. Small traces of starch remain with the intact chloroplasts in the guard cells of the stomata. It is not strange that traces of starch remain in completely yellowed leaves, when we recall that the solution of starch is greatly inhibited by the very low temperatures which often occur suddenly in autumn, and that there is also a high content of insoluble carbohydrate, as shown by chemical analysis.

As to the behavior of the plasma itself, Swart failed to confirm the observation of Kienitz-Gerloff that only disorganized remnants of plasma are to be found in yellowed leaves. The mesophyll cells give the impression of being very poor in plasma, in comparison to the parenchyma of the vascular bundles, but the plasma utricle and the nucleus remain intact. The cells of the leaf, even after leaf-fall, are by no means dead. That a translocation of nitrogenous substance occurs (as the chemical analyses conclusively demonstrated), was also determined by the microscopical study. This may be associated with a destruction of the plasma and a transfer of the disintegration products, but Swart could find no evidence of a translocation of the plasma itself, as held by Kienitz-Gerloff.

In 1860, von Mohl showed the untenableness of Schacht's view that the death and fall of the leaf was caused by the formation of the abscission layer of periderm at the leaf-base, for in many plants this layer does not form, and in the cases where it is present the vascular bundles are not interfered with by its formation. Observations to the same effect by Tison were confirmed by Swart, who considers that there is no longer any doubt that the forma-

tion of the abscission layer does not interfere with the translocation of materials between stem and leaf, although, as Tison demonstrated, thyloses may form in the vessels, and callus in the sieve tubes, but other observed changes (lignification, cork formation and stoppage by wound-gum) take place only after the leaf has fallen.

To test Tison's statements as to the formation of thyloses and callus, Swart employed indigo-carmine, a stain that does not penetrate the protoplast, and is not poisonous (like eosin) in concentrated solution. Branches containing leaves that had begun to turn yellow, as well as green leaves, were placed in deep blue solutions in light, and the penetration of the stain followed by observing cross sections. In all cases, within a few hours or longer (varying according to the species or the length of the branch), the stain had advanced into the vascular bundles of the petiole, and even into the veins of the blades of the yellowed leaves. The stain appeared earlier in the green than in the yellow ones, although the green ones were farther away, and this may be explained by the stoppage of the stomata above described. In how far the closing of the stomata or the changes in the leaf base are to be considered as the primary cause of the diminished suction in the yellow leaves was not determined. The fact remains that any considerable stoppage of the path of liquids is not the rule. The observation that completely yellowed leaves often remain for many days in their exposed places on the tree without becoming dry, leads us to the same conclusion. The author also finds that the leptome elements of the vascular bundles are not cut off by the abscission layer, so that the transportation of substances from the leaf is not hindered in that manner. These facts meet all objections to the transportation theory based on the argument that the abscission layer, being formed before yellowing begins, prevents the passage of materials to and from the leaf.

We are accustomed in our thoughts always to associate leaf fall with a degenerative change of color; but that leaves out of account the periodic or the continual leaf-fall outside of temperate regions, and even often in autumnal leaf-fall, where we observe, especially on certain trees, that many leaves fall when still green, or only half colored. On more careful investigation, however, these latter cases are found to be due to some unfavorable external conditions, such, for example, as frost, which may cause a rupture of the leaf by the formation, at night, in the abscission layer, of ice crystals which thaw on the following day. Other causes mentioned are storms, sudden increase of turgor in the active zone, disease of the roots or of the leaves themselves, and the leaving of collected plants for too long a time in the collecting case. Thus it is seen that plants shed their leaves, not alone when they have ceased their function as organs of nutrition, but also when the life of the plant is threatened, especially as a result of too great transpirational activity.

Mention is made of such trees as the beech, hornbeam and oak, many or most of whose leaves remain on during the winter, on account of their failure to form the abscission layer until the following spring.

The red pigment of the anthocyan group, occurring in solution in the cell-sap, and to which the autumn landscape owes its special charm, is also formed at other seasons. Its appearance before the death of the leaf is restricted to a relatively small number of species, and then it does not replace the yellow pigment, but only masks the latter. It often occurs in mature leaves when there is no outward indication of initial degeneration, such, *e. g.*, as the disintegration of chlorophyll. An active assimilation, combined with a migration of carbohydrates induced by nocturnal cold, is a *conditio sine qua non* for the accumulation of sugar in the cell-sap, which, in turn, is the antecedent condition for the formation of the red pigment. Thus it is that we have the most beautiful red colors in autumn when cold nights alternate with warm days.

There follows a brief discussion of Pick's theory that the presence of anthocyan in leaves

favors the digestion of starch and the translocation of carbohydrates, and especially that it increases the activity of the starch-digesting enzyme. Wortmann disagreed with this, but Stahl, in his researches with variegated leaves, found a satisfactory explanation of the favorable influence of the red pigment on the process of the translocation of materials. The formation of red pigment in leaves in spring and fall, and in high mountains, in summer, is associated with low temperatures which retard the translocation of the photosynthate, and thus decrease the activity of photosynthesis. Stahl's thermoelectric investigations with red-spotted leaves demonstrated an increased absorption of heat in those parts of the leaves containing anthocyan. On the basis of these results, Stahl modified Pick's thesis as follows:

> In the heat-absorbing red coloring matter of leaves the plant possesses a medium for accelerating the transformation of matter and energy.

Contrary to the light-shield theory, which holds that the anthocyan is a protection against the destructive effect of a too intensive light on the chlorophyll, Stahl's theory, especially in view of the favoring of the process of translocation, has the advantage of either giving biological significance to the red pigment in autumn leaves, or of pointing the way to investigations of the metabolism in autumn-red leaves.

It remains to be proved whether the favoring influence of the red pigment on the translocation of material may actually be demonstrated by comparative chemical analyses, or, in other words, whether and in how far red leaves, under the same conditions, suffer a more thorough emptying out than do other leaves on the same plant which have not formed the red pigment. Swartz's analyses of green, yellow and red leaves of *Parottia persica*, with reference to nitrogenous contents showed that, before leaf-fall, the red leaves are more thoroughly emptied than the yellow ones.

If a leaf is dropped from the branch shortly after its color has turned (be the color either yellow, red or white) the cells still remain alive, except in a few cases where *pari passu* with the loss of chlorophyll a brown pigment spreads over the leaf-surface. The outer appearance and also the microscopical characters indicate that the cells by no means contain merely disorganized matter, but maintain their complete vital functions until the appearance of the brown and black color, which indicates the death of the leaf. Thus, with Tswett, Swart concludes that the color change of leaves is not a postmortem decomposition, but a physiological process, and that we have to distinguish two phases in the change of color, viz., the *necrobiotic*, with its yellow, red and white tints, and the postmortem, characterized by the appearance of brown and black color.

But Swart holds that the theory of translocation does not stand and fall with the question as to whether the leaf during yellowing becomes dead or not, for, as he has shown in the investigations here recorded, the translocation of material still takes place from the portions of leaves that have already entered upon change in color. Nevertheless, Swart adds, the theory, in consideration of certain cases, must suffer a certain limitation.

A chapter on final considerations (pp. 97–117) concludes the book.

C. Stuart Gager

SPECIAL ARTICLES

On the Origin of the Loess of Southwestern Indiana [1]

The gallant defenders of the grand old aqueous theory of loess deposition seem to be retreating southward, though their rear-guard vigorously contests every district yielded. Just now they seem to be crossing the Ohio River. Rumor has it that a strip on the north side of the river in southwestern Ohio is still being claimed; southwestern Indiana[2] has been in their undisputed possession for 10 years; and the latest publications on the geology of western Kentucky and southeastern Missouri contain such words as "the writer may state his

[1] Published by permission of the Director of the United States Geological Survey.

[2] *Bull. Geol. Soc. Am.*, Vol. 14, pp. 153–176 and the Patoka folio U. S. Geol. Survey.

belief that the loess of the region under consideration is of fluviatile and not of eolian origin,"[3] and "the submergence during the deposition of the loess."[4]

However, changing the figure, there is reason for suspecting that some have secret longings for a geological statute of limitations which would remove responsibility for statements expressed a few years ago.

Fortunately for the sake of long and interesting debate, it seems impossible to say the last word on the origin of the loess. Nevertheless, some, perhaps weary of the controversy, or perhaps for novelty, have seemed ready to compromise and admit that some loess may have had one origin and some another. Such an interpretation was placed on the loess of southwestern Indiana in the reports just referred to, and the principal object of the present communication is to suggest that another view concerning this material may be tenable.[5] The suggestion is perhaps somewhat tardy, but it has received several years of careful consideration.

The correct interpretation of loess like that of other geologic phenomena depends much on the study of extensive areas. The writer's first-hand knowledge of the loess comes from most of the area of its occurrence in the United States from Nebraska to Ohio, and from Minnesota to Louisiana and also from a part of that in Europe, but it has been particularly in the survey of the Quaternary geology and physiography of a dozen quadrangles in southern Illinois that he has been impressed with the idea that the conclusions set forth with regard to the loess of the Patoka quadrangle are not widely applicable. Naturally, the attempt was made to correlate the work in Illinois with that done in nearby districts, and for this purpose the Patoka area has been visited a half dozen times in as many different years. The object of the later visits was to review the work done in previous years and to make sure that none of the critical places had been missed, though a complete survey was not undertaken.

Some quotations from the Patoka folio will indicate the view there set forth.

> Previous to the present survey of the region no attempt had been made to differentiate the silts, but evidence is now at hand for separating them into two types: (1) thick, yellowish, calcareous, and frequently stratified silts along the immediate borders of the Wabash Valley, which are designated *marl-loess,* and (2) the more clayey, oxidized and structureless silts designated as *common loess,* forming the general mantle over the surface more remote from the river. The first is believed to be of aqueous and the second of eolian derivation.
>
> The marl-loess
>
> occurs at all altitudes from the flood plain to the 500-foot level (120 feet above the river), at which altitude it frequently forms broad terraces and flats . . . burying a rugged rock or till topography. The thickness of the marl-loess in these terraces and flats is sometimes 40 feet or more, but thicknesses of 10 to 200 feet are more common.
>
> The marl-loess is characterized by a high calcareous content and frequently by a sandy texture. Calcareous concretions are exceedingly abundant. In many instances it is delicately stratified and in some cases is interbedded with sands or fine gravels, or even carries scattering pebbles itself.
>
> The perfection of their stratification, their interbedding with sand and gravel, the presence of pebbles in them, their terraced form, and their limitations to the borders of the Wabash point to water as the most probable agent in the accumulation of the marl-loess deposits, the deposition probably being in a fluvio-lacustrine body occupying the lower Wabash Valley, into which the silt was brought from the Iowan ice sheet by the Wabash River.

The view expressed in the *Bulletin of the Geological Society of America,* however, seems, to be that the "marl-loess" is partly eolian and the "common-loess" partly aqueous. To the present writer it does not appear likely that any of the loess of southwestern Indiana

[3] Water Supply paper No. 164 U. S. Geol. Survey, p. 46.

[4] "The Physiographic Development of the Lowlands of Southeast Missouri," p. 30.

[5] The loess is not the only surficial material of the Patoka quadrangle concerning which the writer is inclined to a different view from that set forth in the folio, but it is perhaps the most important.

is water deposited or has any stratification except in certain places where it has a very obscure banding roughly parallel to the present surface such as loess commonly displays.

However, some of the material included under the head of "marl-loess" has a very distinct and approximately horizontal stratification. Most such material is sand of fairly uniform grain, but layers and lenses of gravelly sand, calcareous sand, and also clay and silt are common. The lime is especially abundant in the lower ends of tributary valleys. But, so far as the writer could find, this material is not present at a greater altitude than 440 feet or about fifty feet above the flood plain, and it appears to him to be a part of a widely developed system of valley fillings, remnants of which he has traced through nine states in the upper Mississippi Basin. The tops of the remnants are generally 40–60 feet above the present flood plains. The valley fillings seem to have grown as units notwithstanding the fact that some streams are greatly overloaded and others not at all. It thus happened that some tributaries were ponded by the filling in the main valleys into which they discharged, and the deposits laid down in such comparatively quiet water are fine-grained and calcareous, the lime being largely in the form of small irregular masses differing somewhat from loess kindchen and possibly secreted by plants.

Whatever the origin of the stratified material, surely it is not correct to call it loess, for it differs markedly in several respects from the material known by that name elsewhere, particularly at the type locality in Germany. The principal respects are that it is heterogeneous, being made up of material of all degrees of fineness from clay to gravel, whereas loess is very homogeneous; (2) that it is coextensive with the valley fillings above referred to. Thus the "good sections of a very siliceous form of the marl-loess . . . in the bluffs at Mt. Carmel," the writer would call good exposures of waterlaid terrace sand, having no relation whatever to the true loess.

It seems, further, that some wind-blown sand has been included in the "marl-loess." The valley filling along the Wabash has been the source of much dune sand. Most of the dunes are near the valley bottom, but some of the sand has been carried up on the valley side and some even to the top of the bluff and neighboring divides. This seems to have happened in at least two different epochs, and there appear to have been two or more epochs of loess accumulation, though one is most important. As a result the sand and loess finger into each other in places, but in no section are there more than 2 or 3 layers of each.

It therefore seems to the writer that the "marl-loess" includes

1. Ordinary bluff loess (the principal part).
2. Glacial outwash.
3. Deposits laid down in ponded tributaries.
4. Wind-blown sand.

The stratified and sandy material of classes 2, 3 and 4 are markedly different from true loess.

The division of the loess of the Patoka quadrangle into marl-loess and common-loess appears to have a relation to the fact that on the bluffs near very large streams loess is generally coarser (or at least more free from extremely fine particles) and thicker and more calcareous than at a distance of several miles. The greater part of the material mapped as marl-loess seems to be loess of the ordinary bluff phase. That is to say, it is buff colored, soft, massive, unstratified, homogeneous, calcareous, earth. The particles are mostly angular quartz grains, too small to be classed as sand but some of them are clay. It commonly contains shells of air breathing animals and is characterized further by a tendency to develop and retain vertical cliffs.

The fact that both the bluff-loess and the clay-loess are best developed on the east side of the valley seems more reasonably accounted for by the prevailing westerly winds than by postulating that "the main current hugged the western shore," especially since throughout the Mississippi Basin the loess is more extensive on the east side of the few main streams than on the west.

The writer confesses an inability to see the "terrace form of the marl-loess" as described, the top being said to have a position 500 feet

above sea. As mapped, it lies at all altitudes between 380 and 500, and there are somewhat flattish areas at all altitudes, but especially at 440–450 the altitude of the top of the valley filling in this region. Since divides generally have a rounded profile, and those of this area stand about 500 feet above sea, probably more of the surface here is near that altitude than any other, but the fact is scarcely evident from the topographic maps.

Also the loess does not seem to the writer to show a marked change in character at the 500-foot contour. In color, texture and other physical and chemical characters, including those brought out by mechanical analyses, acid tests and the microscope, the material seems to be a single deposit. In a very rough way the altitude of the surface increases with distance from the river and for this reason the highest "marl-loess" may be, on the whole, a little more like the clayey phase ordinarily found at some distance from larger streams than the average bluff loess, but the difference is slight and there seems to be no noticeable change at the 500 contour.

The statement that the range of fossils "is coextensive with that of the marl-loess, none being found above the 500-foot level" is not in accord with the writer's observations. One rather large collection was made at an altitude of 555 feet, two miles north of the center of Patoka and another at 525, one fourth of a mile northwest of that point. Another collection was made two miles north and one and one half miles west of Owensville at an altitude of 535 feet. At the first named locality the following species, all of which are land shells, were collected: *Pyramidula alternata* Say, *Succinea* sp. (young), *Helicodiscus multilineatus* Say, *Euconulus trochiformis* Montague, *Polygyra monodon* Rock, *P. hirsuta* Say, *Pupa muscorum* L., *Helicina occulta* Say.

The maximum thickness of the "marl-loess" is only about 40 feet, and yet it is continuous except where it has been subjected to severe erosion, as on the steeper valley sides. If the "marl-loess" were water deposited—a valley filling now dissected—one might reasonably expect to find remnants of triangular cross section and limited extent located where the meandering streams had chanced to leave them, instead of an almost continuous layer covering valley sides. The "marl-loess" as mapped has a vertical range of 120 feet or more, but the maximum thickness is scarcely a third as much. Like true loess, it appears to mantle hill and valley alike, in places obliterating minor irregularities, but nowhere greatly modifying the major features.

The writer found no pebbles in place in the material which he would class as loess, and none in any of the material described as marl-loess at a greater altitude than 440 or at most 450 feet. Pebbles are, to be sure, pretty good evidence that a deposit is not eolian, providing they are certainly in place. But there are so many ways in which pebbles are scattered that unless they were certainly in place or very nearly in place, it would seem unsafe to regard them as evidence of water deposition.

The relation of the true loess to the general form of the surface of southern Illinois throws a rather important sidelight on the problem. The Mississippi and Ohio are bordered by high rough country and the general surface slope is not toward these two principal streams but away from them. The loess, however, here as elsewhere, is thick, porous, calcareous and fossiliferous on the high hills near the rivers and gradually becomes thin and clayey toward the low country at some distance from them. If the part below 500 feet were water deposited, thousands of square miles of the interior lowland must have been submerged and this area should presumably have a considerable deposit if not a thicker one than the higher country, but the deposit is thin or wanting in the lowland. It thickens gradually toward the rivers and is thickest on the hills where the water must have been most shallow, along the Ohio and Mississippi. The bluff loess certainly appears to be a single deposit from the bases of the hills at 375 or 400 feet to their crests at 700 or 800 feet where it is thickest. The distribution of the loess is very different from that of material known to be waterlaid valley filling.

Briefly, the writer believes that the so-called

marl loess of southwestern Indiana consists of wind deposited true loess, stream laid valley filling, and dune sand, and that the true loess part of it together with the "common loess" corresponds to the well-known loess of other parts of the Mississippi basin.

Eugene Wesley Shaw

SOCIETIES AND ACADEMIES

ST. LOUIS ACADEMY OF SCIENCES

At a meeting of the Academy of Sciences of St. Louis, held October 19, Professor Nipher gave an account of his work during the summer of 1914, at his summer place in Hessel, Mich., on "Magnetic Disturbances in the Earth's Field due to Dynamite Explosions, Burning Black Powder and the Fog-horn of a Steamer."

The magnetic needle was mounted on a frame of timber, the vertical posts of which were set two feet into the ground. The frame and the boxes containing the control magnets were loaded with half a ton of rocks. The boxes containing the control magnets were also clamped to the frame with large wooden clamps. The base of the air-tight vessel containing the magnetic needle was also clamped to the frame. The torsion head was braced by means of wooden bars. Four cords at right angles to each other were attached to the torsion head. They passed outward and downward through holes in the table, and upon them were hung two bars of wood at right angles to each other. These bars were also loaded with rock. The apparatus was protected from heat effects by a series of blankets within the tent, having air-spaces between.

No difficulty was found in producing marked local disturbances in the earth's field by means of dynamite suspended in air to the east and to the north of the tent. The needle being at right angles to the magnetic meridian, this disturbance indicated a variation in the horizontal intensity.

The amount of dynamite exploded varied from half a stick six or eight feet from the tent, to thirty sticks distant 275 feet. In the larger explosions, the sticks were placed end to end on bars of wood having a cross section $1 \times 1\frac{1}{2}$ inch. Each stick of dynamite was securely held in place upon the bar by means of a winding of heavy cord. The ends of the bar suffered no appreciable injury. Those parts in contact with the dynamite vanished in dust so fine that it was difficult to find any trace of it. The changes in the position of the needle in explosions of this character amounted to ten or fifteen minutes of arc.

Much more marked effects were produced by distributing half a stick of dynamite into a column about 15 feet in length. It was packed closely into the angle of a little trough of wood, which rested upon a heavy beam of wood. The trough was held in position by means of masses of rock hung on cords. The column of dynamite was in line with the needle and either above or below the level of the needle. In this way deflections of about one degree of arc were produced. The direction of deflection was reversed by reversing the direction in which the explosion traversed the column. The end of the column nearest the needle was distant from it about ten feet.

This seems to indicate that a magnetic field is created around this exploding column, like that which exists around a wire conductor carrying a current of electricity. In most of the experiments of this character only a small part of the column exploded. It is believed that the conditions which will cause an explosion of the entire column with equal violence throughout have been finally attained, and this work will be continued.

The black powder was spread over a platform having an area of 25 square feet, placed a few feet from the west side of the tent. The amount of powder spread over the platform was from 25 to 50 pounds. The flame shot upward to a height of 15 to 30 feet. The lines of the earth's field were deflected around the region filled by this flame. The intensity of field within the tent was momentarily increased. The deflection of the needle amounted to from 25 to 50 minutes of arc.

At the request of Professor Nipher, the captains of the steamers of the Arnold Transit Co. were instructed by the president, Mr. Geo. T. Arnold, to blow a loud and prolonged blast on their fog-horns when at the nearest point to the observing station. This distance was about half a mile. Appreciable effects were thus produced when the air was quiet, the sky was clear and the intensity of the field had reached a high value. The result in every case was to decrease the intensity of the earth's field.

In two cases the blast from the 5 o'clock boat was at once followed by a premature appearance of the sunset disturbance. Rhythmical vibrations over from 10 to 20 degrees of arc at once followed, and continued for four or five minutes. This result needs further examination under more favorable conditions.

C. H. Darforth,
Recording Secretary